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AND

THE ARTS.

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𝔍llustrated with 𝔈ngrabings.

BY WILLIAM NICHOLSON.

ADVERTISEMENT.

THE Authors of Original Papers in the present Volume, are Mr. John Gough; Rev. Jonathan Wilson; E. O.; Mr. Wilson; Amicus; Mr. G. B. Greenough; Hon. Robert Clifford; A. A.; Mr. William Irvine; James Thompson, M. D.; Mr. J. Dalton; J. P.; Sir Henry C. Englefield, Bart. F. R. S.; Thomas Young, M. D. F. R. S.; Mr. John Antis; Ra. Thicknesse, Esq.; R. B; H. G.; Mr. William Henry; James Malton, Esq.; Mr. A. G. Theolden; Mr. W. Boswell; Alexander Marcet, M. D.; Mr. Timothy Sheldrake; Mr. Ezekiel Walker; C. Wilkinson, Esq.; Mr. Charles Sylvester; A Correspondent; Mr. Edward Troughton; W. Jessop, Esq.; C. L.; Thomas Harrison, Esq.

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And of English Memoirs, abridged or extracted; Charles Hatchett, Esq. F. R. S.; Benjamin Count of Rumford, V. P. R. S. &c.; Thomas Young, M. D. F. R. S.; Mr. John Prior; Sir James Hall, Bart.; Mr. W. Bowler; Mr. Thomas Willis.

Of the Engravings the Subjects are, 1. Two Plates of Apparatus for Experiments on Heat; by Count Rumford. 2.

A new

ADVERTISEMENT.

A new Electrical Instrument, by Mr. Wilson. 3. New Filtering Apparatus, by Professor Parrot. 4. Original Blowpipe, by the Abbé Melograni. 5. Apparatus for Filtration, by Sir H. C. Englefield, Bart, &c. F. R. S. 6. Structure for purifying an entire Stream of Water. 7. Very simple striking Part of a Clock, by Mr. Prior. 8. An Instrument for counting the Number of Draughts from a Mine, by Mr. Antis. 9. An Instrument for delineating in Perspective. 10. Cheap Apparatus for drawing Ovals. 11. Sketch of Mr. Malton's Method of making very large Port-folios for Drawings and Prints. 12. A Lamp for burning Tallow, by Mr. J. W. Boswell. 13. An Evaporating Furnace, by Curadau. 14. A Gripe for the Safety of Carriages, by Mr. Bowler. 15. A perpetual First Mover. 16. A new Parallel Rule. 17. A new Compensation Pendulum, by Mr. Edward Troughton. 18. An Instrument for measuring the Absorption of Gases, by Morozzo. 19. Diagram to explain the Galvanic Energy upon Water, by C. Wilkinson, Esq. 20. Two Plates exhibiting a Condenser of Force, by which the Action of a variable First Mover is converted into a constant Pressure, by Prony.

Soho Square, December, 1801.

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ARTICLE I.

Letter from Mr. JOHN GOUGH, *containing a Narrative of some less common Effects of Lightning, by the Rev.* JONATHAN WILSON, *and Remarks by the Communicator.*

SIR, *Middleshaw, Aug.* 13, 1804.

THE following is a copy of a letter which I lately received from the Rev. Jonathan Wilson, Vicar of the parish of Biddulph, in Staffordshire. The facts and remarks contained in it, may prove acceptable to many of your philosophical readers, as well as to myself; and by giving it a place in your Journal, you will oblige

JOHN GOUGH.

Congleton, Aug. 1, 1804.

THE following circumstance induced me to defer my reply to your letter for a few days: On the 15th ult. I was informed in my way to church, by the farmer of Biddulph Hall, that, on the 7th, the lightning struck a plain arable field of his, which he uses at present for a cow pasture. I went with him, and found that the electric fluid had killed the tops of the taller thistles, in a circle of about twenty yards in diameter; but

Stroke of lightning in a plain field, affecting a circle of 20 yards in diameter.

...e herbage and fmall thiftles, no higher than the grafs, did ...ot appear to have been finged at all. Thofe in the centre, ...or about three yards, feemed much lefs injured than thofe nearer the circumference. In two places the foil was turned up, a few feet in length and an inch or two in depth; and where the ground ceafed to be broken up, one might perceive that the lightning had glanced, with feveral ramifications, along the furface, but under the long grafs, leaving a track, fuch as moles and mice fometimes make. In one place, where the impreffion made upon the foil was deepeft, and fomewhat refembled the letter V, a ftraight round hole appeared at the angle, which was about two feet deep, and about three inches in diameter.

Probability of a ftone having fallen from the atmofphere. After I had left the place, it occurred to me that this hole might have been produced by a ftone falling from the clouds, a phenomenon which has lately caufed much fpeculation. The more I thought of this, the more probable it feemed. The hole was round and perpendicular, at no great diftance from the end of the barn, and a tall tree was alfo near the place; either of which was more likely to attract the lightning than the plain field. A very lofty tower, which was ftruck a few years ago, was about three times the diftance of the tree. The thunder was not fo loud as when the tower was ftruck, but the fmell of fulphur was much ftronger. Thefe confiderations determined me to return the firft opportunity and examine the place more thoroughly.

On examination none was found. From one caufe or other I could not do this conveniently before yefterday evening, when the farmer and his fon very obligingly went with me, and dug a circular pit about four feet in diameter, having the hole in its centre. At the depth of two feet they came at a fhivery rock of grit, in which I hoped to find the expected ftone imbedded; but when the foil was cleared away level with the bottom of the hole, there was no fuch thing; nothing appeared but fmall, oblong, flat pieces of gritftone, through the moift crevices of which the electric fluid probably efcaped. Though difappointed in the principal part that I hoped to communicate, I refolved to fend you the above, as I think the lightning very feldom defcends fo far into folid ground, unlefs when guided by fome particular conductor. If the account afford you any information or amufement,

I fhall

I shall be glad; and should the subject merit any of your queries, they will be received with pleasure by

Your humble servant,

JONATHAN WILSON.

Remarks on the above.

THE first part of Mr. Wilson's letter countenances Dr. Darwin's explanation of fairy-rings; for of all the thistles included in the electrified circle, those in the centre received the least injury. The hypothesis of Dr. Darwin has always seemed to me to labour under unsurmountable difficulties; and this is the reason why I point out a fact which establishes an exact agreement of the explanation, and natural appearances, in one instance.

Fairy-rings are permanent objects, the nature of which is but imperfectly understood; and it is to be wished that Mr. Wilson would find leisure to observe and describe the future consequences of the electric discharge at Biddulph Hall. The herbage of the ramified figure mentioned in the letter, will in all likelihood die in the course of a few weeks; because plants that have sustained strong electrical shocks, seldom survive the operation more than a month or two. Should this change take place in the circle, it will give a new appearance to the surface of the ground, by exposing to view the branched path of the lightning; nor is it improbable but that this alteration will be succeeded by another of a more singular kind. There is reason to suspect, that each ramification of the track will be again covered with a lively verdure the next spring, being accompanied at the same time by a contiguous patch of blasted herbage resembling its own figure. This suspicion is countenanced by certain observations of Dr. Hutton of Edinburgh, who remarked, that the fairy-rings upon Arthur's Seat annually increase in diameter; i. e. the withered circumference of each circle becomes green in spring, and is surrounded in a short time by a fresh ring of a russet colour. We have no right to dispute the justice of Dr. Hutton's observations, but the question is undetermined, whether the appearance is constant, or results from the nature of the soil. The perusal of the foregoing remarks will perhaps admonish my friend Mr. Wilson, that Fortune has furnished him with an opportunity

of

of profecuting the inquiry, fuch as fhe rarely affords to perfons of correct obfervation. I will not trefpafs upon your pages by a comment on the remaining facts of my friend's letter; let them fpeak for themfelves; in particular, the preference of a thunder cloud to low ground, in the prefence of lofty objects, including a tall tree.

JOHN GOUGH.

ANNOTATION.

I BELIEVE the opinion that fairy-rings are caufed by lightning, is of confiderable antiquity. Dr. Prieftley gave fupport to this conclufion by his experiment of the concentric rings formed on a polifhed metallic furface by the explofion of a battery. An effect of this kind produced in Kenfington Gardens, is defcribed, with an engraving, in our Quarto Series, Vol. I. p. 546.

W. N.

II.

On the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

Computation of fquares and cubes.

I SHOULD not have troubled you with any remarks on the method of computing fquares and cubes, if I had not feen the paper which was printed on that fubject in your Journal, for laft July. But as it appears, from that letter, that there may be perfons who would like to employ themfelves on thefe calculations, it is defirable that the plaineft and eafieft methods fhould be pointed out to them.

Every one who is accuftomed to calculation, is acquainted with the advantage of conftantly repeating the fame operations. When the refult of any calculation is to be made out, in fome parts by multiplication and in others by divifion, fubtraction, or addition, it is impoffible for the moft unwearied diligence to avoid occafional miftakes. It is right, therefore, to fimplify as much as poffible in this refpect; and if the calculation

lation cannot be effected by a fingle operation, I believe I fhall be joined by all perfons accuftomed to arithmetical computation, in recommending the ufe of addition * in preference to fubtraction, and of multiplication in preference to divifion. Your correfpondent H. G. does not feem to have paid fufficient attention to thefe confiderations; and I fear that he not only would be foon wearied, if he were to work by the method which he recommends, but (what is of ftill more confequence) he could not depend for any continuance upon the accuracy of his computations.

The only method by which an extenfive table like Mr. Councer's could have been calculated, muft have been by the conftant addition of differences; and the rules for this method may be eafily deduced from the following confiderations: Let $x+3a$, $x+2a$, $x+a$, &c. be any numbers in arithmetical progreffion. Then by the binomial theorem,

$$\overline{x+3a}\,|^2 = x^2 + 6ax + 9a^2$$

$$\overline{x+2a}\,|^2 = x^2 + 4ax + 4a^2$$

$$\overline{x+a}\,|^2 = x^2 + 2ax + a^2$$

1ſt Differences.	2d Differences.
$2ax + 5a^2$	
	$2a^2$
$2ax + 3a^2$	
	$2a^2$
$2ax + a^2$	

It will be foreign from my prefent purpofe to enter into a particular confideration of the manner in which thefe differences arife. It will be fufficient to remark, that the fecond difference is conftant, and that its value will be always 2, when we confider a as $= 1$, or the progreffion $x + 3a$, $x + 2a$, &c. as a feries of common numbers differing from one another by unity. Hence the firft differences of the fquares of fuch numbers become themfelves an arithmetical progreffion, of which the common difference is 2; and confequently, if we know the firft difference between the fquares of any two fuch numbers, we can find the difference between the fquares of the two next.

* If this opinion wanted any argument in its favour, I might fupport it by the authority of that able mathematician Captain Mendoza, who has calculated his new tables in fuch a manner as to make all the equations additional.

What has been faid may be exemplified by applying it to the continuation of Mr. Councer's fquares.   For

$$798684121 \text{ is the fquare of } 28261,$$
$$798627600 \text{ is the fquare of } 28260,$$

56521 is their difference, therefore 56523 is the difference between the fquare of 28261 and the fquare of 28262.   By the addition in every inftance of 2, we may find the feries of differences; and by the conftant addition of them, we may eafily find the fquares.   Thus

$$
\begin{array}{l}
798684121 = \overline{28261}|^2 \\
\quad 56523 \\
\hline
798740644 = \overline{28262}|^2 \\
\quad 56525 \\
\hline
798797169 = \overline{28263}|^2 \\
\quad 56527 \\
\hline
798853696 = \overline{28264}|^2 \\
\quad 56529 \\
\hline
798910225 = \overline{28265}|^2
\end{array}
$$

By a fimilar method the table of cubes might be continued by the addition of differences; but, in this cafe, the calculation will be a little more complicated : Becaufe, if the terms of any arithmetical progreffion be raifed to the nth power, there will be n orders of differences, and the laft will be the only one of them which will be conftant.   For the cube, therefore, where $n = 3$, we fhall have one order of differences more than for the fquare.

As before, let the progreffion be $x + 3a$, $x + 2a$, $x + a$, x, &c.

	1st Difference.	2d Difference.	3d Diff.
$\overline{x+4a}\rvert^3 = x^3 + 12ax^2 + 48a^2x + 64a^3$	$3ax^2 + 21a^2x + 37a^3$	$6a^2x + 18a^3$	$6a^3$
$\overline{x+3a}\rvert^3 = x^3 + 9ax^2 + 27a^2x + 27a^3$	$3ax^2 + 15a^2x + 19a^3$	$6a^2x + 12a^3$	$6a^3$
$\overline{x+2a}\rvert^3 = x^3 + 6ax^2 + 12a^2x + 8a^3$	$3ax^2 + 9a^2x + 7a^3$	$6a^2x + 6a^3$	
$\overline{x+a}\rvert^3 = x^3 + 3ax^2 + 3a^2x + a^3$	$3ax^2 + 3a^2x + a^3$		
$x^3 = x^3$			

If therefore we make $a = 1$, the third difference will be constantly 6, and the second difference will become $6x + 18$, $6x + 12$, $6x + 6$; so that if any three numbers be taken in an arithmetical series (in which $a = 1$), the second difference of their cubes will be constantly six times the middle number.

For the sake of example, we may apply these observations to the continuation of Mr. Councer's cubes. We may consider 26559, 26560, 26561, as part of an arithmetical series: by the table we could find the difference of the cubes of 26559

and

and 26560, and, by what has been obferved above, the fecond difference of the three cubes will be * $6.26560 = 159360$; which, added to the difference between the cubes of 26559 and 26560, will give the difference between the cubes of 26560 and 26561. So far we fhould proceed exactly in the fteps which are traced out by your correfpondent H. G.; but I differ from him in this refpect, that I fhould by no means recommend this method for the folitary calculation of any particular cube: it is only mentioned here for the fake of fhewing how to afcertain the numbers by which we muft commence the feries of differences, when we want to continue a table which has already been calculated to a certain extent. For the third difference is conftantly 6; therefore if we take an arithmetical progreffion, of which the terms differ from one another by that number, and which begins with 159360 (fuch as 159360, 159366, 159372, &c. &c.), we fhall afcertain the fecond differences of the cubes which we want to find. Thefe fecond differences muft be added to the refpective firft differences, and, by that means, we fhall find a feries of numbers, which, added to each particular cube, will give us the cubes of the numbers next above it. Thus

$$\overline{26560}\vert^3 = 18736316416000$$

$$\overline{26559}\vert^3 = 18734200194879$$

$$2116221121 = \overline{26560}\vert^3 - \overline{26559}\vert^3$$
159360 2d Difference.

$$2116380481 = \overline{26561}\vert^3 - \overline{26560}\vert^3$$
159366 2d Difference.

$$2116539847 = \overline{26562}\vert^3 - \overline{26561}\vert^3$$
159372 2d D.

$$2116699219 = \overline{26563}\vert^3 - \overline{26562}\vert^3$$
159378 2d D.

$$2116858597 = \overline{26564}\vert^3 - \overline{26563}\vert^3$$
159384 2d D.

$$2117017981 = \overline{26565}\vert^3 - \overline{26564}\vert^3$$

Having thus afcertained the firft differences, we may proceed to add them to the cubes:

* The dot (.) between the figures is here ufed as the fign of multiplication.

N.

$$18736316416000 = \overline{26560}|^3$$
$$2116380431 \text{ 1ft Diff.}$$

$$18738432796481 = \overline{26561}|^3$$
$$2116539847 \text{ 1ft D.}$$

$$18740549336328 = \overline{26562}|^3$$
$$2116699219 \text{ 1ft D.}$$

$$18742666035547 = \overline{26563}|^3$$
$$2116858597 \text{ 1ft D.}$$

$$18744782894144 = \overline{26564}|^3$$
$$2117017981 \text{ 1ft D.}$$

$$18746899912125 = \overline{26565}|^3$$

The methods here ftated are, probably, the eafieft which can be devifed for conftructing an extenfive table: but it muft frequently happen, that the calculator will want the fquare or cube of fome number which is greater than any which is contained in the table. It may be ufeful, therefore, to confider the affiftance which the table may afford him in facilitating the computation.

It is well known, that $x^m \times y^m = \overline{xy}|^m$. * Therefore, if we want to find the fquare of a number which is e double of any contained in the table, we have only to multiply the given fquare by 4. In the fame manner, if we want to find the fquare of a number which is exactly three or four times as great as any contained in the table, we may find it by multiplying the given fquare by 9 or 16. Thus, for example,

$$16522331600 = \text{ the fquare of } 128540;$$
$$4$$

$$66090126400 = \text{ the fquare of } 257080;$$

$$16522274521 = \text{ the fquare of } 128539;$$

$$148700470689 = \text{ the fquare of } 385617.$$

* I do not know whether it is worth while to mention the circumftance, but your correfpondent H. G. has made a miftake in the application of this rule. For he fays that, if we " multiply the cube of any given root by 8, the product will be the cube of twice the next root:" whereas the product will be the cube of twice the given root.

In

In this manner we may find the fquares of all even numbers which do not exceed the double of the table : we may alfo find the fquares of numbers, to a greater extent, which are multiples of 3 : the multiples of 4 and 5 may be fquared likewife, if we multiply by 16 and 25 ; but fquare numbers increafe very rapidly, and confequently this method can be only ufed (with advantage) for the fmall multiples of the tabular numbers. We muft, therefore, confider fome other method which may be applicable to prime numbers and multiples of large ones.

The fquare of $2x + 1 = 4x^2 + 4x + 1 = 2x^2 + 4x + 2 + 2x^2 - 1 = 2.\overline{x+1}^2 + 2x^2 - 1$, or $2.\overline{x+1}^2 + x^2 - 1$. If, therefore, we want to find the fquare of an odd number which does not exceed the double of thofe which are fquared in the table, we muft divide the next even number below it by 2, and this half will be equivalent to x in the above equation ; the next number above this half will give us $x + 1$. Having thefe two fmaller numbers, we may find their fquares by the table ; and the fum of their fquares multiplied by 2, will exceed the fquare required by 1. Thus, if it were required to find the fquare of $257079 = 2.128539 + 1$: Here $x = 128539$, and $x + 1 = 128540$; therefore, by Mr. Councer's table, the fquare of 128540 would be 16522531600

the fquare of 128539 would be 16522274521

33044806121

2

66089612242

Therefore the fquare of 257079 is 66089612241.

Inftead of examining each particular cafe, when the number to be fquared is more than the double of thofe in the table, it will be beft to confider the theorem, from which a general rule may be deduced. $\overline{nx + a}^2 = n^2x^2 + 2anx + a^2 = nx^2 + 2anx + na^2 + \overline{n^2 - n}.x^2 + \overline{1 - n}.a^2 = n.\overline{x+a}^2 + n.\overline{n-1}.x^2 + \overline{1 - n}.a^2 = n.\overline{x+a}^2 + \overline{n-1}.x^2 + \overline{1 - n}.a^2$. Hence we muft multiply x^2 by $\overline{n-1}$, to the product add $\overline{x+a}^2$, multiply the fum of thefe two quantities by n, and this laft product will exceed the fquare required by $\overline{n-1}.a^2$. The only caution neceffary, is in dividing the given number by x; for as

we

we fuppofe it equal to $nx + a$, we fhould make x as great as it poffibly can be, without being fo large, that when added to the remainder a (as $x + a$), it fhould exceed the numbers of which the fquares are given in the table. Becaufe n and a are the only numbers by which we have to multiply, and as a muft be lefs than n, we fhall always have them as fmall as poffible, if we take x according to the directions here given. If, for example, it be required to find the fquare of 385618, which is greater than twice, and lefs than three times 128540 (the greateft number which is fquared by Mr. Councer's table); here $385618 = 3.128539 + 1$; therefore if $n = 3$, $x = 128539$, and $a = 1$, $x + a$ will be $= 128540$, and will be within the extent of the table : Hence

x^2, or the fquare of 128539, would be by the table   16522274521

$n - 1$, or $3 - 1$ - -   2

                                     33044549042

$\overline{x + a}^2$, or the fquare of 128540, would be   16522531600

                                     49567080642

n or 3 - -   3

                                     148701241926

and as $\overline{n - 1}.a^2 = 2$, the fquare of 385618 will be 148701241924

If the fquare required were that of 385619, in this cafe $a = 2$, and confequently, if $x = 128539$, $x + a$, or 128541, would exceed the extent of the table : therefore n muft, in this cafe, be equal to 4, $x = 96404$, and $a = 3$, and

the fquare of 96404 = 9293731216

                                     3

                                 27881193648

the fquare of 96407 = 9294309649

                                 37175503297

                                     4

                           148702013188; but $\overline{n - 1}.a^2 =$

3.9, or 27 ; therefore the }   148702013161.
fquare of 385619 is   }

From this example we fee the neceffity of being careful that $x + a$ does not exceed the numbers in the table : at the

                                             fame

fame time it may be remarked, that this is an extreme cafe, which can never occur but under particular circumftances: for it only happens when the number to be fquared is lefs than a multiple of the higheft number in the table, and greater than the fame multiple of the number next lefs than the higheft; that is, for a table like Mr. Councer's, it muft be lefs than $m.128540$, and greater than $m.128539$: under thefe circum-ftances, if a be greater than 1, the cafe will occur which is the fubject of the above caution.

It remains for us to confider the method of finding the cubes of numbers which exceed thofe given in the table.

Upon the principle to which we referred before, that $x^m \times y^m = \overline{xy}|^m$, we may find the cube of a number which is a multiple of any one contained in the table, by fimple multiplication: for the cube of any number multiplied by 8, will give the cube of double that number; the cube multiplied by 29, 64, or 125, will give us the cube of 3, 4, or 5 times the number: Thus,

$$
\begin{array}{r}
3368928644271 \\
8 \\
\hline
26951429154168 \\
\end{array}
= \text{ the cube of } 14991
$$

$$= \text{ the cube of } 29982.$$

$$
\begin{array}{r}
1045678375000 \\
27 \\
\hline
73197486625000 \\
209135672000 \\
\hline
28233316125000 \\
\end{array}
= \text{ the cube of } 10150
$$

$$28233316125000 = \text{ the cube of } 30150.$$

But as the cubes increafe more rapidly even than the fquares, it will ftill be more neceffary in this cafe than in the former, to eftablifh fome means of finding the cubes of thofe high numbers which are either prime or not exactly a fmall multiple of a number contained in the table; and by proceeding in a manner fimilar to that which we ufed for the fquares, we eafily eftablifh a general rule for this purpofe: For $\overline{nx+a}|^3 = n^3x^3 + 3an^2x^2 + 3a^2nx + a^3 = nx^3 + 3nax^2 + 3na^2x + na^3 + \overline{n^3 - n}$. $x^3 + \overline{3an^2 - 3an} \cdot x^2 + a^3 - na^3 = n.\overline{x+a}|^3 + n.\overline{a^2 - 1}.x^3 + n. \overline{n-1}.3ax^2 + \overline{1-n}.a^3 = n \times \overline{x+a}|^3 + \overline{n-1}.\overline{n+1} x^3 + 3ax^2 + \overline{1-n}.a^3$. Determine, therefore, n, x, and a, in the fame manner

manner as for the fquare, and be equally careful that $x + a$ does not exceed the numbers in the table; add $\overline{n+1} . x^3$ to $3ax^2$; multiply the fum of thefe two quantities by $n-1$; to the product add $\overline{x+a})^3$, and multiply this fum by n : this laft product will exceed the cube required by $\overline{n-1}.a^3$.

If, for example, it be required to find the cube of 53119, or $2.26559 + 1$: Then $n = 2, a = 1, x = 26559$, and $x + a = 26560$, and the cube of $53119 = 2 \times$

$\overline{26560})^3 + 3.\overline{26559})^3 + 3.\overline{26559})^2 - 1$: Therefore, by Mr. Councer's table, the cube of 26559 would be 18734200194879

The fquare of 26559 -	705380481

	18734905575360
	3

	56204716726080
The cube of 26560 -	18736316416000

	74941033142080
	2

	149882066284160
therefore the cube of 53119 is -	149882066284159

Laftly, let it be required to find the cube of 79601, or $3.26533 + 2$: here $n = 3, a = 2, x = 26533, x + a = 26535$; therefore, by what has been demonftrated, $\overline{79601})^3$

$= 3 \times \overline{26535})^3 + 2 \times 4.\overline{26533})^3 + 6.\overline{26533})^3) - 2.8.$

$\overline{26533})^2 = 704000089$		$\overline{26533})^3 =$	18679234361437
3			2

2112000267	

37358468722874
2112000267

37360580723141
4

149442322492564
$\overline{26535})^3 = 18683458680375$

168125781572939
3

504377314718817
Therefore the cube of 79601 is -   501377344718801

E. O.

Analyfis

III.

Analyſis of a Triple Sulphuret of Lead, Antimony, and Copper, from Cornwall. By CHARLES HATCHET, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

Hiſtory of the mineral.

THE ſubſtance which forms the ſubject of this paper, has hitherto been regarded as an ore of antimony; it is extremely rare, and has only been obtained from Huel Boys, in the pariſh of Endellion, a mine which, from deficiency of profit, has for ſome time been abandoned.

The ſcarcity of the ore has probably been the cauſe of its being unknown to foreign mineralogiſts; indeed few even of the Britiſh cabinets poſſeſs it; but the moſt perfect and beautiful ſpecimens are (as far as I know) to be ſeen in the ſplendid collection of Philip Raſhleigh, Eſq. of Menabilly, in Cornwall.

To Mr. Raſhleigh we are indebted for the firſt deſcription of this ore *; but no ſubſequent notice had been taken of it, until the preceding paper was written by the Count de Bournon, whoſe eminent merits, as a mineralogiſt and cryſtallographer, are well known to this Society.

I.

Sp. gravity.

The ſpecific gravity of this ſubſtance is 5766, 65° of Fahrenheit.

II.

Heat by the blow-pipe expelled ſulphur and white fumes, and left ſulphuret of lead and metallic copper.

If ſuddenly heated on charcoal, by the blowpipe, it crackles and ſplits; but, when gradually expoſed to the flame, it liquifies, and, upon cooling, aſſumes a dull metallic grey colour.

When the globule was longer expoſed to heat, white fumes (which at firſt had a ſulphureous odour) were evolved, and partly ſettled on the charcoal.

Ebullition prevailed during the diſcharge of theſe white fumes; and the globule gradually ſuffered conſiderable diminution, remaining at length tranquil, and of a very dark gray colour.

* Specimens of Britiſh Minerals, ſelected from the Cabinet of Philip Raſhleigh, Eſq. F. R. S. &c. Part I. page 34, Plate XIX.

Upon

Upon examination, this appeared to be principally fulphuret of lead, which, like a cruft, enveloped a minute globule of metallic copper, fo malleable as to bear to be flattened by a hammer.

III.

Some of the ore, finely powdered, was put into a matrafs, and nitric acid diluted with an equal portion of water was poured on it. Upon being digefted in a low heat, a confiderable part, was diffolved, with much effervefcence. Some fulphur, which floated, was feparated ; and the clear liquor, which was bluifh green, was decanted from the refiduum at the bottom of the veffel. *Digeftion with dilute nitrous acid, &c. indicated fulphur, lead, copper, and antimony, with a little iron.*

A great part of the excefs of acid being expelled from the folution, it was largely diluted with diftilled water, and fome diffolved muriate of foda was added ; but this did not produce any alteration in the tranfparency of the liquor. A folution of fulphate of foda was then poured in, and formed a very copious precipitate of fulphate of lead.

When this had been feparated, the liquor was faturated with ammonia ; by which it was changed to a deep blue colour. A few flocculi of iron were feparated ; and the remainder was found to contain nothing but copper.

The fulphur which had floated, was added to the refiduum which had fubfided to the bottom of the matrafs; and the whole was digefted with muriatic acid. This folution was of a ftraw colour; and, when feparated from the fulphur, and poured into a large quantity of water, afforded a plentiful white precipitate.

This precipitate was completely refolved into white fumes, by the blowpipe; and the muriatic folution of it, when added to water impregnated with hydro-fulphuret of ammonia, formed the orange coloured precipitate, commonly known by the appellation of golden fulphur of antimony.

IV.

Muriatic acid did not immediately act upon the pulverized ore ; but a folution was fpeedily effected by the addition of a few drops of nitric acid : pure fulphur was feparated ; and the liquor, being decanted into water, yielded a copious precipitate of oxide of antimony. *The fame refults by treatment with muriatic acid and a little nitric.*

4

The

The filtrated solution, by gradual evaporation, afforded cryftals of muriate of lead; and the lead which afterwards remained in the liquor, was feparated by a few drops of fulphuric acid.

The folution was now of a bright green colour, and, as before, was found only to contain copper, and a minute portion of iron; the former was therefore precipitated in the metallic ftate, by a plate of zinc.

Thefe experiments, with others which I have not thought neceffary to mention, prove, that the conftituent parts of this ore are lead, antimony, copper, and a little iron, combined with fulphur; and, when the fpecific gravity, the external and internal colour, fracture, grain, and other characters are confidered, there can be no doubt but that at leaft the three firft metals exift in the ore, in, or nearly in, the metallic ftate, combined with fulphur, fo as to form a triple fulphuret; to afcertain the proportions of which, the following analyfis was made.

V.   Analyfis.

Analyfis. Pulverizing.   A. 200 grains of the ore, reduced to a fine powder, were put into a glafs matrafs, and, two ounces of muriatic acid being added, the veffel was placed in a fand-bath. As this acid, even when heated, fcarcely produced any effect, fome nitric acid was gradually added, by drops, until a moderate effervefcence began to appear.

Digeftion with nitro-mur. acid,   The whole was then digefted in a gentle heat, during one hour; and a green coloured folution was formed, whilft a quantity of fulphur floated on the furface, which was collected, and was again digefted in another veffel, with half an ounce of muriatic acid.

left fulphur   The fulphur then appeared to be pure, and, being well wafhed and dried on bibulous paper, weighed 34 grains: it was afterwards burned in a porcelain cup, without leaving any other refidium than a flight dark ftain.

The green folution afforded, by dilution, oxide of antimony,   B. The green folution, by cooling, had depofited a white faline fediment; but this difappeared upon the application of heat, and the addition of the muriatic acid in which the fulphur had been digefted.

The folution was perfectly tranfparent, and of a yellowifh green: it was made to boil, and in this ftate was added to three

quarts

quarts of boiling diftilled water, which immediately became like milk; this was poured on a very bibulous filter, fo that the liquor paffed through before it had time to cool; and the white precipitate thus collected, being well edulcorated with boiling water, and dried on a fand-bath, weighed 63 grains.

C. The wafhings were added to the filtrated liquor; and the whole was gradually evaporated at different times, between each of which it was fuffered to cool, and remain undifturbed during feveral hours. A quantity of cryftallized muriate of lead was thus obtained, until nearly the whole of the liquor was evaporated: to this laft portion a few drops of fulphuric acid were added, and the evaporation was carried on to drynefs; after which the refiduum, being diffolved in boiling diftilled water, left a fmall portion of fulphate of lead. *and by fubfe-quent evapora-tion muriate of lead.*

The cryftallized muriate of lead was then diffolved in boiling water; and, being precipitated by fulphate of foda, was added •to the former portion, was wafhed, dried on a fand-bath, and then weighed 120.20 grains.

D. The filtrated liquor was now of a pale bluifh-green, which changed to deep blue, upon the addition of ammonia; fome ochraceous flocculi were collected, and, when dry, were heated with wax in a porcelain crucible, by which they became completely attractable by the magnet, and weighed 2.40 grains. *Ammonia threw down a little iron.*

E. The clear blue liquor was evaporated nearly to drynefs; and, being boiled with ftrong lixivium of pure potafh, until the whole was almoft reduced to a dry mafs, it was diffolved in boiling diftilled water; and the black oxide of copper, being collected and wafhed on a filter, was completely dried, and weighed 32 grains. *The blue liquor evapor. and boil-ed with potafh and then diffol-ved in water gave black copper.*

200 grains of the ore, treated as here ftated, afforded,

	Grains.	
A. Sulphur - - • -	34.	
B.• Oxide of antimony - -	63.	Product.
C. Sulphate of lead - -	120.20	
D. Iron - - - -	2.40	
E. Black oxide of copper -	32.	

But the metals compofing this triple fulphuret are evidently in the metallic ftate; and white oxide of antimony precipitated

from muriatic acid by water, is to metallic antimony as 130 to 100; therefore, the 63 grains of the oxide muft be eftimated at 48.46, grains of the metal.

Again, fulphate of lead is to metallic lead as 141 to 100; therefore, 120.20 grains of the former are = 85.24 grains of the latter. And, laftly, black oxide of copper contains 20 *per cent.* of oxygen; confequently, 32 grains of the black oxide are = 25.60 grains of metallic copper.

Component parts.

The proportions for 200 grains of the ore, will therefore be,

Sulphur	-	-	-	-	34.
Antimony	-	-	-	-	48.46
Lead	-	-	-	-	85.24
Iron	-	-	-	-	2.40
Copper	-	-	-	-	25.60
					195.70
			Lofs	-	4.30

Or, *per cent.*

Sulphur	-	-	-	-	17.
Antimony	-	-	-	-	24.23
Lead	-	-	-	-	42.62
Iron	-	-	-	-	1.20
Copper	-	-	-	-	12.80
					97.85
			Lofs	-	2.15

Thefe proportions, I have reafon to believe, are tolerably exaff; for I did not obferve any effential variation in the refults of two other analyfes, which I made of this fubftance, with every poffible precaution.

The lofs may be principally afcribed to the oxide of antimony and fulphate of lead; but efpecially to the former, which has a great tendency to adhere to filters and glafs veffels.

In fome of the preliminary experiments, I obtained a fmall portion of zinc; but, having received, through the kindnefs of Mr. R. Phillips, of Lombard-ftreet, fome pure cryftals of the ore, I found that the zinc had proceeded from blende, which was imperceptibly mixed in the fpecimens which I had firft examined.

Defcription

IV.

Defcription of a compound Electrical Inftrument for condenfing and doubling: with Experiments.   By Mr. WILSON.

To Mr. NICHOLSON,

SIR,

I TAKE the liberty of troubling you with the following ac- Compound count and drawing of a compound condenfer of electricity, electrical con= which I think is an improvement on Cavallo's multiplier of denfer. electricity, defcribed in his Treatife on Electricity in the third volume, of the fourth edition, and if you think it worth a place in your Philofophical Journal, you will very much oblige me by inferting it therein.

I am your obedient
Humble fervant,
W. WILSON.

The drawing is an exact reprefentation of the inftrument, Defcription and about half its real fize, *(Plate 3.)*   The plates A, B, C, D, drawing of the and E are fupported by glafs fticks, covered with fealing wax inftrument. at the upper part; but the plate F is fupported by a wire, which has a joint at the bottom, by which it may be brought near the plate E, or thrown back away from it.   3 is a fcrew, which ftops againft the glafs fupport of E, and regulates the diftance of the two plates when one is brought near the other. The plates A, C and E are fixed to the bottom board, and B and D to the levers L l, which move round the pins p p, and and are connected by the rod R.   It will be feen by the draw-ing, that when the lever L is moved, the plates B and D are moved in contrary directions, that is, they both approach to-wards or recede from the plate C at the fame time, but on op-pofite fides.   The faces of the plates are ground flat, and they are fo adjufted that if the fcrews 2 2 did not ftop the levers when they are moved, the face of B would ftop flat againft the face of A, and fo would the face of D againft C.   The fcrews 2 2 ftop them when at a very fmall diftance, which may be made more or lefs by turning the fcrews.

C 2   This

This instrument serves the purpose of a condenser, a single and double multiplier, and a doubler of electricity. When it is used as a single condenser, the plate B is brought as near A as the screw 2 will permit, and then the wires *b* and *f* touch one another and uninsulate B, which increases the capacity of A. As the capacity of A is increased it will receive a greater charge of electricity from any electrified body brought into contact with it. Consequently, when B is removed, the charge on A will be much more intense than it would have been if the plate B had not been opposed to it; but this intensity will not be greater than that of the electrified body, if its surface is not greater than the surface of the receiving plate A; for a condenser does not increase the quantity of electricity, it only collects it into a smaller space than it was before. So that if the electricity of a very small body is required to be ascertained, the simple condenser will not answer the purpose. Some other means must be used when this is the case, and Cavallo's multiplier is the least exceptionable of the instruments used for this purpose. The instrument represented in the drawing forms a multiplier, either single or double, for when B is near A and uninsulated, A will receive a much greater charge that it would if B had not been near. And because B is uninsulated, A will induce a contrary state on B, and of nearly equal intensity; which state is preserved: For the instant the lever is moved to carry B farther from A; the contact between the wires *b* and *f* is discontinued, and B insulated, and as it removes farther away from A, the intensity of its charge will increase the same as that of A will, and be ready to part with nearly the whole to another condenser.

Now as the plate B removes away from A, the plate D approaches C, and when at a small distance from it, the wires *d* and *g* come into contact, which operation uninsulates D, and therefore causes the two plates C and D to form a condenser; and at the same instant that *d* touches *g*, the wires *b* and *c* touch one another, so that nearly the whole of the charge of B is communicated to C, which induces a contrary state on D, as the charge of A did on B. D will therefore be in the same state as A is, which state is preserved; for the instant the plate B removes away from near C, the plate D also removes away from it, and the contact between the wires *d* and *g* is discontinued, and D insulated; and by continuing the motion the

plate

plate B will come near A, and D will remove away from C *Its ufes as a*
till the wires *b* and *f* touch one another, at which inftant the *condenfer, a*
wire *d* touches the wire *e*. Now if the plate E and F are *fingle and double*
near one another, they will form a third condenfer, and nearly *multiplier, and*
the whole of the charge of D is communicated to E in the *a doubler of electricity.*
fame manner the charge of B was communicated to C ; at the
fame time B recruits its charge by coming near A, which will
be communicated to C, which induces a contrary ftate on D,
and which D communicates to E, and this operation is re-
peated every time the lever is moved backwards and forwards;
fo that the charge on C is increafing at every motion of the
lever, while the charge on A remains the fame ; and when C
becomes fo much charged as not to be capable of receiving
any more from B (which will be the cafe in a certain number
of motions) the operation will ftill go on between D and E,
the charge on E increafing while the charge on C remains the
fame, fo that E will acquire a charge as much greater than C
as the charge on C is greater than the charge on A, which will
be manifefted by removing the plate F away from E. When
the wire *a* is fcrewed into the plate A, and connected with E
by means of the wire 4 5; the inftrument then poffeffes all the
properties of the double of electricity, for then all the charge
communicated to E (which is of the fame nature as that of A)
will be communicated to A, which will continually increafe the
intenfity of its charge, and that will have an increafed effect
on B, &c. at every motion of the lever. There is no limits to
this accumulation, but that where the charge is fo intenfe as to
pafs from one plate to the other in the form of a fpark.

I have made many experiments with this inftrument relative *On the fpon-*
to its fpontaneous electricity, and I find, as a fingle multiplier *taneous elec-*
(that is when an electrometer is connected with C) it has no *tricity of this*
effect on the moft delicate gold leaf electrometer I could make; *inftrument.*
but as a double multiplier (that is when an electrometer is
connected with E) there is fome fmall effect if fome electricity
had been communicated to it within an hour or two, although
it may have been difcharged by touching each of the plates with
a metal point (which I find is the moft effectual way of dif-
charging fmall portions of electricity,) but if it has ftood three
or four hours after being difcharged, it will not give any figns
of electricity. When ufed as a doubler it always becomes
electrified with between eight and fixteen motions of the lever,

even

On the fpon-
taneous elec-
tricity of this
inftrument.

even though it has not been ufed for two or three months, —
But if it has been ufed within two or three hours, the effect
will take place with fewer motions of the lever, and if it has
been ufed within a few minutes, two or three motions will be
more than fufficient to afcertain the quality of the electricity.
It is to be obferved that the inftrument was always difcharged
(by the metal point as before obferved) between each trial.

In the courfe of making the experiments on the fpontaneous
electricity of this inftrument I found that it was always pofi-
tive if the inftrument had not been ufed for two or three days,
whatever electricity it was laft charged with.   But the time it
muft ftand unufed after it has been difcharged, to produce this
effect, depends a great deal on the weather; if the air is very
humid, twenty-four hours is quite fufficient, but if it is very
dry, it will require four or five days.

After I had obferved that after the inftrument was charged
with pofitive electricity its fpontaneous electricity was always
pofitive; and that after it was charged with negative electri-
city, its fpontaneous electricity was negative only within a
certain time after it had been difcharged, and then became
pofitive; and alfo that it required a greater number of motions
of the lever to produce a certain effect on the electrometer
with negative electricity, the longer it ftood after it had been
charged with negative electricity, and that when it became
pofitive, the longer it ftood the lefs number of motions of the
lever it required to effect the electrometer with pofitive elec-
tricity to a certain degree, and this within fome certain limits.
I was at a lofs how to account for this change.   However,
after fome confideration, I began to fufpect that the plates (al-
though all of the fame metal, copper) had each a property
peculiar to itfelf, of acquiring a certain fmall charge of one
kind of electricity in preference to the other; and that if they
were left to themfelves they would naturally do fo.   I therefore
began a fet of experiments to afcertain the probability of this
fuppofition.   Firft, directly after the inftrument had been ufed
for negative electricity, I difcharged it by touching each of the
plates with a metal point, which I held in contact with each
plate for two or three feconds.   An electrometer was then
connected with the plate A, and while it was in this fituation,
it was made to diverge with pofitive electricity ;* it was then

* The electrometer was made to diverge by bringing an excited
glafs or fealing wax near it.

dif-

difcharged by a flight touch with the finger, the lever was then moved backwards and forwards about fourteen times, and the electrometer diverged with negative elect. icity. This was repeated feveral times with the fame effect.

The lever was then put in fuch a pofition that the plates could not be fuppofed to act as condenfers, one pair more than another, and the whole was left untouched for twenty-four hours. It was then tried, and with twenty motions of the lever the electrometer diverged with pofitive electricity.

It was then left untouched for eighteen hours more, and on trial the electrometer diverged with pofitive electricity. The inftrument was next difcharged with the metal point as before, and the electrometer made to diverge with negative electricity, which was difcharged by a flight touch of the finger. The lever was now worked, and with nine motions of the electrometer diverged with pofitive electricity. It was then difcharged, and left untouched for fixteen hours, after which it diverged with pofitive electricity after a few motions of the lever. It was then left untouched for five days, after that time it gave pofitive electricity when the lever was worked. Thefe experiments I have repeated a great many times with the fame effect.

As it appeared from the foregoing experiments that the refiduum on the electrometer was infufficient to overcome the effect of the refiduum on the plates, I was induced to charge the whole inftrument with the effect of the electricity, I would wifh to communicate to it, and this I did by making the electrometer diverge with the defired electricity, and while it was divergent I began working the lever, to communicate the effect to all the plates. I charged the inftrument with negative electricity firft, and difcharged it with the metal point. The lever was then worked, and in eight motions the electrometer diverged with negative electricity. It was then left untouched for five days, when eighteen motions of the lever made the electrometer diverge with pofitive electricity. This has been often repeated with the fame effect.

The inftrument was next charged with pofitive electricity, and difcharged as before, after which, fix motions of the lever made the electrometer diverge with pofitive electricity. It was left untouched for twenty-four hours, when the elec-
trometer

On the fpon-
taneous elec-
tricity of this
inftrument.

trometer diverged with pofitive electricity with twelve motions of the lever, and fo it did after it had flood untouched for fix days.

The probability of the foregoing fuppofition concerning the property of the plates acquiring a charge of one electricity in preference to the other, was more ftrengthened by the following experiment, which has been repeated feveral times. The inftrument was charged with negative electricity, and then left without difcharging for twenty-four hours, when fixteen motions of the lever made the electrometer diverge with pofitive electricity.

I make no doubt if the plates were of different metals this effect would be more ftriking, and that in fome cafes we fhould have the contrary electricity. I intend to conftruct one of thefe inftruments in fuch a way that plates of any metal may be put on, and their effect tried. I think this a fubject well worth purfuing as it may throw fome light on fome of the phenomena of electricity, which have their caufes at prefent buried in obfcurity.

V.

*Note refpecting the Sufpenfion of Zinc in Hidrogen, and the con-
fequent Ignition and Fufion of Platina Wire. By* AMICUS.

To Mr. NICHOLSON.

DEAR SIR,

Ignition and
fufion of platina
in Cuthbertfon's
gazometer,

I EMPLOY a gafometer of a much more fimple conftruction and much lefs expence than Lavoifier's, which that fkilful and intelligent artift, *Cuthbertfon*, invented at my entreaty, about ten years ago. It is defcribed in your valuable Journal (quarto feries, vol. II. p. 235.) The brafs rod or the thick wire, which conveys the electric fire through the upper receiver, has about half an inch of platina wire appended, to hang juft over the aperture through which the hydrogen gas rifes at the bottom in the brafs work. Zinc of any other fort of metal would either be melted or readily oxided. But the other day, I was much furprized to fee the plati___ on becoming as ufual *red hot* from the flame of the hy___gen gas, melt into globules

as steadily as copper or brafs wire would have done.   I foon —caufed by the
perceived from the *colour* of the flame of the hydrogen and volatilization of
the depofit of *white clouds* on the infide of the glafs, that the zinc, &c.
rapid folution of the finely powdered zinc ufed to afford the
gas, that this metal was actually fufpended or diffolved in the
gas, and hence the zinc acting to the platina fufed the latter.
It may be perhaps ufeful to know this fact, which occurred
during the public lecture.

<div align="center">Your's, as ufual,

AMICUS.</div>

July 2, 1804.

<div align="center">

VI.

Defcription of a Blow-Pipe acting by the Preffure of Water, by the
* Abbé Melograni.   In a Letter from Mr.* G. B. GREENOUGH.

To Mr. NICHOLSON.
</div>

SIR,

<div align="center">

No. 15, *Parliament-Street,*
Auguft 13, 1804.
</div>

PERMIT me to make you acquainted with the principle of Blow-pipe fup-
an inftrument invented by the Abbé Melograni, and ufed by plied by two vef-
him at the Royal Mineralogical Collection at Naples, as a felb alternately
fubftitute for the blow-pipe.   I regret that I cannot fend you changing their
an accurate account of its conftruction, as I made no fketch pofitions.
at the time, and fpeak only from recollection.

Two hollow glafs globes of convenient fize were connected
together by two brafs tubes laid along-fide each other, one of
them having a valve or ftop cock, I am not fure which, at
each end, and a fide tube of the fame metal going off from the
middle at right angles.   The frame was attached to the verti-
cal tubes, fo as to allow the globes to circumvolve each other.
Let the lower globe be half filled with water and inverted;
Then the water in A (*Fig.* 2. *Plate* IV.) running into B
through the tube C, will force a conftant ftream of air into
the tube D, and thence, the upper orifice being clofed, into
the fide-tube E, at the mouth of which the candle is placed.
When the water is nearly run out invert the globes.

<div align="right">If</div>

If the principle of this inftrument fhall appear to you de‑ ferving of the public attention, it would give me pleafure to fee it fubmitted to the readers of your excellent Journal. I have the honour to fubfcribe myfelf,

SIR,

Your obedient humble fervant,

G. B. GREENOUGH.

VII.

Outlines of the Mineralogical Syftems of Romé de Lifle and the Abbé Hauy: with Obfervations. By the Abbé Buée. Communicated by the Hon. ROBERT CLIFFORD.

To Mr. NICHOLSON.

SIR,

Outlines of the mineralogical fyftems of Romé de Lifle, and the Abbé Hauy.

WHEN the public mind appears at any time to direct it‑ felf towards a particular fcience (as it does at prefent towards mineralogy,) no perfon can feel the neceffity of removing any erroneous opinions relating to that fcience, more forcibly than a gentleman who dedicates his time, as you do, Sir, to pro‑ pagate correct fcience by means of your Journal. Actuated by the fame fentiments, may I requeft the infertion of the en‑ clofed paper, whofe object is to remove feveral erroneous opinions relating to the mineralogical fyftems of Romé de L'Ifle, and of the Abbé Hauy. It was written by the Abbé Buée in French for a friend of his, but on its being communicated to me by the author, I requefted leave to fend an Englifh tranf‑ lation of it to you, thinking it might be acceptable to thofe who are fond of philofophical purfuits. Should your opinion coincide with mine, you will certainly afford it a place in one of your enfuing numbers.

And I remain, Sir,

Your very humble fervant,

ROBERT CLIFFORD.

Welbeck-Street, July 13, 1804.

A Letter from the Abbé Buée to Mr. —, on Mr. Romé de L'Isle's and the Abbé Hauy's Theories of Cryftallography.

SIR,

IN confequence of your requeft, I fend you the parallel of the two Theories of Cryftallography, which feem to divide mineralogifts in this country, thofe of Mr. Romé de L'Ifle and of the Abbé Hauy. You are perfeҫtly acquainted with the former theory, but nearly a ftranger to the latter. Having lived for fix and thirty years in habits of intimacy with the Abbé, I dwell with pleafure on his works, and will do my utmoft to fatisfy your curiofity.

Parallel between the theories of Romé de l'Ifle and Hauy.

To Mr. de L'Ifle is due the merit of having called the at-tention of naturalifts to that negleҫted branch of mineralogy, cryftallography; of having difcovered that that branch, though negleҫted, was perhaps the moft interefting part of mineralo-gy, and the only part which could raife it to the dignity of a correҫt fcience; in fhort, of having difcovered order, by nu-merous obfervations as ingenious as new, there, where a Cronftet, a Bergman, a Buffon, or a Kirwan could perceive nothing but confufion; and thus feemed to refcue nature from the charge of caprice, almoft imputed to it becaufe great mi-neralogifts had negleҫted to ftudy its unerring laws.

De l'Ifle firft exhibited order in cryftallogra-phy.

It was exclufively referved to the Abbé Hauy to point out, to explain, and apply thofe laws. He demonftrated where De L'Ifle affirmed. He difcovered thofe *hidden faҫts*, which he has fince fhewn to be the mathematical confequences of faҫts obferved by De L'Ifle. If the latter furnifhed a part of the materials, the Abbé has augmented and employed them. The difcoveries of thefe two writers, force me to fubdivide cryftallography into two diftinҫt parts; defcriptive and phi-lofophical; and under thefe two heads I will rapidly defcribe the labours of each author.

Hauy firft ex-plained the laws of cryftallization.

Defcriptive.—The moft important part of Mr. de L'Ifle's work confifts in his cryftallographical tables. In each of thefe tables (feven in number) he defcribes one of the principal forms affumed by cryftals, and then delineates the different modifications of which that form is fufceptible, by means of dif-ferent troncations *(troncature)* as he calls them.

Defcriptive cry-ftallography. De l'Ifle's tables.

For elucidation, let us take a cube, the primitive form of the fecond table. A cube, it is known, has fix faces, eight folid

Modifications of cryftals defcribed by troncations. Inftance the cube.

folid angles, and twelve edges. If the cube be truncated in a parallel to one of its faces, a rectangled parallelipipedon will be produced, and the equality of the faces will be deftroyed.

If the eight folid angles of the cube be ftruck off, eight new faces will replace the eight folid angles, and in place of fix fides we fhall have fourteen. If the twelve edges be taken off, twelve new faces will fucceed the ftrait lines, and the folid will have eighteen fides. Such are De L'Ifle's *fimple troncations*. They may be then combined with each other, and made more or lefs deep; hence an immenfe variety of new figures. But thefe new forms again may be truncated in the directions either of their faces, folid angles, or edges, and thefe new troncations more or lefs deep, called by De L'Ifle *fur-troncatures*, may alfo be combined with each other. Here the forms muft multiply to infinity, and their boundlefs numbers will foon bury the primitive cube in oblivion.

It muft not be fuppofed that nature has furnifhed us with this infinite feries of forms; indeed Mr. De L'Ifle in his tables has only mentioned thofe he had obferved, with fome few additional fuppofitious figures, of which feveral have been fince difcovered to exift.

Account of De l'Ifle's feven tables of cryftals. This ingenious naturalift has given us, as I have already faid, feven cryftallographical tables. In the 1ft he defcribes the tetrahedron and its modifications; in the 2d the cube; in the 3d the rectangular octahedron; in the 4th the rhomboidal parallelipipedon; in the 5th the rhomboidal octahedron; in the 6th the dodecahedron, with triangular faces, and to each are fubjoined their refpective modifications. The object of the 7th table is to point out certain modifications of the octahedron and parallelipipedon, whether rectangular or rhomboidal. Plates accompany each table, where the figures are drawn, and in the obfervations and notes on them are to be found the meafures of the principal angles.

They contain general folids; which are fubfequently applied to the cryftals of nature, viz. falts, ftones, and metals. Thefe cryftallographical tables exhibit only general reprefentations of folids, which Mr. de L'Ifle in the courfe of his work applies to the different cryftals which had already been difcovered, and fallen within his obfervation. His work confifts of three parts. In the firft he treats of faline cryftals; in the fecond of ftoney *(pierreux)* cryftals, and in the third of metallic cryftals. Thofe of the firft clafs are artificial, thofe of the two latter claffes are natural cryftals, and are fubdivided into genera, fpecies, and varieties.

When

When treating of a species or of a variety, he refers his reader to the table where the figure of that species or variety is to be found, and he then enumerates every thing relating to minerals affuming that cryftalline form: But I cannot terminate this fketch better than by the following extract from the Abbé Hauy's treatife on mineralogy. *His method of defcription.*

" In fhort Romé de L'Ifle reduced the ftudy of cryftallography to principles more exact and more confiftent with obfervation. He claffed together, as much as he was able, cryftals of the fame nature. From among the different forms belonging to each fpecies, he felected one which appeared to him to be the moft proper, on account of its fimplicity, for the primitive form, and then fuppofing it to be truncated in different manners, he deduced the other forms, and eftablifhed a certain gradation or feries of paffages from the primitive form to that of polyëdrons which would fcarcely appear to have any connection with it. To the defcriptions and figures which he gave of the cryftalline forms, he added the mechanical meafurement of the principal angles, and he fhewed (a moft effential point) that thefe angles were conftantly the fame in each variety. In a word, his cryftallography is the fruit of immenfe labour by its extent; almoft entirely new in its object, and of great value for its utility." (Vol I. page 17.) *Quotation exhibiting a fummary of De l'Ifle's labours.*

The Abbé Hauy in his treatife on mineralogy embraces a far greater extent than Mr. de L'Ifle. His mineralogy is not only defcriptive, but it is phyfical, chymical and geometrical. In the perfuafion that a mineral cannot be well defcribed, nor even in many cafes recognized, unlefs its phyfical, chemical, and geometrical characters are clearly laid down, the Abbé never omits any one of thofe characters when afcertained, and expofes with the moft fcrupulous exactnefs every thing relating to them that obfervation has authenticated. He has beftowed particular attention to the electrical and magnetic phenomena, and has enriched the fcience with a multitude of new and curious obfervations. He attentively examined the property of double refraction, which feveral tranfparent minerals enjoy; and here again he has extended the boundaries of fcience. A few minerals were known to poffefs this property, and the Abbé has difcovered it in feveral where it had never been furmifed. *The mineralogy of Hauy is not merely defcriptive, but phyfical, chemical and geometrical. Electricity. Magnetifm. Double refraction.*

When we confider that writers on mineralogy have hitherto grounded their fyftems *exclufively*, fome on the exterior characters, *Hauy has excluded none of the habitudes,*

racters, others on the chemical properties of minerals, and that the Abbé really has, purfuant to his plan, (fee in the beginning of the volume of plates, *La diftribution methodique des mineraux, par clasſes, ordres, Genres et eſpeces,* the methodical diftribution of minerals into clasſes, orders, genera, and ſpecies) united all that has hitherto been difcovered on mineralogy, without falling into that confufion which has ever been imputed to other mineralogical writers, we are almoft aftonifhed at his fuccefs. " To clafs minerals, to furnifh the means of difcovering to which clafs, genus, and ſpecies a mineral under examination belongs," are the two great problems which the Abbé Hauy propofes for folution.

He folves the firft in following Bergman's method (founded on chemical properties) much improved by the immenfe progrefs which chemical analyfis has made fince the days of that great chemift. In the folution of the fecond he follows Werner's method (grounded on exterior characters) but corroborated by a multitude of new experiments, eafily made and brought to a furprizing degree of correctnefs by the Abbé's own labours on the forms of cryftals. But I perceive that the immenfity of matter contained in this treatife is leading me from that point which I had particularly in view, I mean cryftallography.

In the defcription of cryftals the Abbé employs three different means. 1ſt He draws their figure; he does not give cryftallographical tables as Romé de L'Ifle, which are only general properties, but draws feparately each ſpecies and variety. Every form given in the plates has been examined by himfelf; he has calculated every angle, and neverthelefs his plates contain one third more figures than De L'Ifles tables.

2d He makes ufe of fymbolic figns, than which nothing can be more fimple, and were invented not to recall the form of the cryftal to the mind, but the laws by which it has been produced. Yet I have met with perfons, who were fo accuftomed to thefe figns, that at firft fight of them they could immediately figure to themfelves the form of the correfponding cryftals. Thefe figns can alfo be fpoken, and much circumlocution in confequence avoided. 3d A fignificant nomenclature fubdivided into general and particular. The general is for the *minerals,* and comprifes only fubftantives; the particular for the *cryftals,* and is entirely compofed of adjectives. He ftudioufly avoided

introducing new names, and neverthelefs has been obliged to introduce many, where new fubftances, names capable of giving falfe impreffions, or others void of fignification and un-fupported by long ufage, required it. He then fubftituted names taken from the Greek; a language, he fays, that eminently enjoys the faculty of combining feveral words together, fo as to form one reprefenting concifely the object to be named.— The adjectives ufed in the nomenclature of the cryftals alfo al-lude to fome remarkable circumftance of the cryftalline form.

I fhall now proceed to philofophical cryftallography, which might be called the philofophy of mineralogy. It does not confift in fearching for the primary caufes of phenomena, no-thing can be lefs philofophical than fuch a refearch; primary caufes will ever be beyond the reach of the human mind! The immortal Newton was the firft to point out to us by the method followed in his admirable book of the Principia, that the only true philofophical way of treating a phyfical fcience, or of explaining a natural fact, was to demonftrate that it was the mathematical confequence of a general law, grounded on an aggregate of facts already obferved and capable of correct calculation. If any one of thefe conditions are wanting, we immediately launch out into hypothefis, expla-nations become vague, and however much we may be per-fuaded of the truth of our affertions, we can acquire no cer-tainty. *Philofophical cryftallography.*

The true method of explaining phyfics is to fhew that the facts are mathematically deducible from a general law. Hypothefis.

Let us apply thefe principles to our two writers. De L'Ifle, in declaring that the various forms obferved in cryftals of the fame fubftance are only modifications of one conftant primitive form, certainly announced a moft important truth. It was a flafh of genius; but in a philofophical enquiry, to prove it and not fimply to fay it, was the neceffary ftep. On the firft in-fpection of his cryftallographical tables, a ftudent is tempted to think that important truth demonftrated; but on a clofer ex-amination, the impreffion is done away. The fame order per-vades every table. By flight paffages the ftudent is led from the fimpler to the more compound forms, and after every paf-fage, is tempted to fay; this can only be a modification of the primitive; then when the real cryftals, and the figures of the tables are compared together, and all thofe of the fame fpecies (with a very few exceptions) are found in the fame table, how eafy it is to perfuade ourfelves that nature *muft* operate by *The method of Rome de l'Ifle fhewn to be de-ficient in cor-rectnefs.*

fimilar

fimilar paffages when producing the various forms of cryftals; and the primitive of the table before us *muft* be the primitive of the cryftal under examination. In a word it is the moft fimple form ; and firft impreffions greatly ftrengthen the illufion. If perfuafion was the fole objeƈt of philofophy, De L'Ifle would have been a powerful philofopher; but philofophy muft convince, demonftrate, and wreft confent, however violently oppofed. An enemy muft not therefore be able to make ufe of the fame arms, or adduce the fame proofs to eftablifh a contrary opinion. Neverthelefs fuch would be cafe with Mr.

De L'Ifle's tables and the application of them: For it is an inconteftable faƈt, that by a feries of arbitrary troncations we may pafs infenfibly from any given form to any other. Grounded on this principle, and feconded by Mr. de L'Ifle's ingenuity, any form may become primitive, and any other deduced from it. Now as the combinations are infinite, a multitude of tables may be conftruƈted, forms of the fame fpecies may be difperfed in different tables: the moft fimple of each table will be the primitive, therefore forms of the fame fpecies will have different primitives. But when by the fame principle both fides of the queftion can be proved, nothing is proved.

To fay the moft fimple form muft be the primitive is an illufion, for we know not what is the moft fimple for nature. With our feeble organs and confined fenfes we can form no judgment of *fimple*, when the operations of nature are in queftion. Nature embraces the entire univerfe, her laws are fimple; but the combinations made according to thofe laws are unbounded, therefore complicated.

Let us not forget, however, that the idea of troncations, and the idea of taking the moft fimple form for the primitive, are fo natural that they muft have been the firft to prefent themfelves to the man who was opening the carreer: " Often, fays the Abbé Hauy, (vol. I. page 14) a more compoufid form only differs from a more fimple one by certain little faces which may be produced by feƈtions, either at the folid angles, or on the edges of the fimpler form," and in a note he fays, " this was the obfervation which gave the celebrated Romé de L'Ifle the idea of his fyftem of troncations, that he might fucceffively deduce from each other the different varieties of cryftalline forms affumed by the fame fubftance."

Mr.

Mr. De L'Ifle terminates the introduction to his work by certain axioms, as he ftyles them, the 2d and 16th are as follow:

" II. Every angular polyhedron, or every cryftallized fubftance is a SALT in the moft extended acceptation of that term."

" XVI. Every faline fubftance whofe conftituent parts are perfectly faturated and combined affects the *cubic* form, or its inverfe the *octahedron;* whereas the falts which are not neuter, or whofe conftituent parts are not exactly combined, affect either the *prijmatic* or the *rhomboidal* forms."

I need fcarcely obferve that, to treat thefe axioms only as doubtful, would be treating them kindly. The other axioms are matters of fact, from which he draws no confequence, and indeed it would have been difficult for him to have drawn any.

The Abbé Hauy does not undertake to prove generally, that among the different cryftalline forms of the fame fubftance, one of them is the primitive; but he produces that primitive form from each cryftal, which is always fimilar in fimilar fubftances. He demonftrates it analytically and fynthetically; by an ana- lyfis which might be called *mineralogical analyfis,* and pointed out by nature herfelf: By a fynthefis hitherto the property of mathematicians, but here fupported by the general laws which his analyfis has revealed to him. The conftant accord found between this fynthefis, and daily obfervation is a proof of the exactnefs of his method.

Two facts were the foundation of his theory: 1. In all times jewellers and lapidaries have remarked that ftones are more eafily cut in fome certain directions than in others. 2. Whoever has been in the habit of feeing natural cryftals muft have obferved, that when their forms are well-determined, they are always terminated by plane furfaces. Thus, fays the Abbé, " thofe foft outlines, and that roundnefs fo frequent in the animal and the vegetable kingdom, where they are inherent to the organization, and contribute even to the elegance of the forms, indicate on the contrary in minerals a want of perfection. The characteriftic of true beauty in minerals is the ftrait line, and it was with truth that Romé de L'Ifle declared *that line* to be the peculiar property of the mineral kingdom." The firft fact fuggefted the mineralogical analyfis, and the fecond furnifhed him with the laws on which he grounded his fynthefis.

Enquiries on the firſt Faƈt.

On the analyſis or ſubdiviſion of cryſtals into regularly formed portions by fiſſure.

1. All cryſtals that can be ſplit by means of inſtruments, offer to the view, if ſplit in certain directions, plane and ſmooth ſurfaces. If divided in other directions, the fracture is rugged. I uſe the word *ſplit*, and not *ſawed* or *cut*, as the ſections of the cryſtal are not to be obtained by ſlow and continued efforts, but by ſudden ſhocks. Patience, dexterity, and habit enabled the Abbé to ſplit a great number of cryſtals; in all he diſcovered plane ſmooth ſurfaces when ſplit in certain directions, but when in other directions, the fracture was always rugged and irregular. I requeſt, Sir, your attention to this important fact, it is fundamental, and the more important, as ſeveral perſons of much general information have neglected to attend to it, and, in conſequence, have ſuppoſed the whole of this theory to be grounded on hypotheſis. It would be equally erroneous to confound theſe ſections of cryſtals with De L'Iſle's troncations. The latter indeed warns his readers, that by the word *troncations* he wiſhes only to figure the appearance of the'cryſtal examined. They are not therefore real, but only a means of warping the imagination to the exterior form of the cryſtal, and are by their nature only deſcriptive. The Abbé Hauy's ſections are real, and are pointed out to the obſerver by the interior ſtructure of the cryſtals; they are experimental.

It is *experimental*, whereas the troncations of De l'Iſle were *hypothetical*.

The inclinations of the faces are conſtant.

2. The plane ſmooth ſurfaces obtained by the above method are reſpectively parallel to 3, 4 or 6 planes. The mutual inclination of theſe planes to each other are conſtant in cryſtals of the ſame ſubſtance, whatever may be the exterior form of the cryſtal. Native antimony, phoſphate of lead, and quartz ſeem to ſhew an appearance of more than ſix planes, and the Abbé Hauy leans to the opinion of only five planes in ſome caſes; but as theſe are exceptions to the general rule, and would only tend to complicate this ſtatement, I ſhall take no further notice of them.

Explanation of the figures and formation of cryſtals by the method of Hauy.

Let us ſuppoſe the ſmooth ſurfaces to be only parallel to three planes, or in other words, that the ſubſtance will only ſplit in three directions, in that caſe the ſections can only produce a parallelipipedon, whoſe nature is determined by the mutual inclination of the planes to each other. If the planes are perpendicular, it will be rectangular, &c.

We next ſuppoſe the ſmooth ſurfaces to be parallel to four planes. Here a diſtinction ariſes, whether three of theſe planes have

have

have a common interfection or not; and it muft be remembered that if the four planes have a common interfection, no folid can be produced, as they can neither bound nor include a fpace. If therefore three of the four planes have a common interfection, the fplittings will produce either one hexahedral prifm, or three parallelipipedons, which will be fimilar or diffimilar, according to the fimilarity or diffimilarity of inclination of the planes, or one triangular prifm. On the contrary, if the four planes only interfect each other two and two, there will be produced either one octahedron, or four parallelipipedons, or one tetrahedron.

Laftly, let us fuppofe the fmooth furfaces to be parallel to fix planes; then there arife an immenfe number of cafes.— But we will for the prefent confine ourfelves to the only cafe -that has hitherto been obferved in nature: Where the interfection of the planes is two and two, then we obtain either, 1. dodecahedron with pentagonal, quadrilateral or triangular fides, according to the fections made, or fifteen octahedrons, or twenty parallelipipedons, or fifteen tetrahedrons. It may be proper to obferve here that though the fections parallel to the fix planes may be clearly indicated, neverthelefs it rarely happens they can all be executed, but it will fuffice for the purpofes of geometry that they be clearly indicated to render the confequences drawn from them mathematically correct.

Having laid down thefe premifes, let us proceed to the diffection of a cryftal of carbonate of lime (the fpath calcaire of De L'Ifle) whofe primitive form is a rhomboid or a parallelipipedon bounded by rhombs. Hitherto fections have only been obtained in the three directions parallel to its fides. If thefe fections be directed fo as to always pafs through the center of two oppofite fides, they will produce eight rhomboids equal to each other, and fimilar to the original one. The fame operation may be repeated on each of thefe eight rhomboids, and continued fo long as the fubftance remains carbonate of lime, that is to fay, to be a combination of 55 parts of lime, 34 of carbonic acid, and 11 parts of water of cryftallization:— (fee Bergman.) But this divifion of the cryftals into fimilar folids has a term, beyond which we fhould come to the fmalleft particles of the body, which could not be divided without chemical decompofition; that is to fay, without an alteration

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Explanation of
the figures and
formation of
cryftals by the
method of Hauy.
in the proportions of lime, carbonic acid, and water. Thefe
laft particles which are ftill rhomboids, are what the Abbé
Hauy calls the *integrant particles* of the carbonate of lime. In
the fuppofition therefore that a rhomboid of this fubftance can
only be divided in three directions, by fections parallel to the
fides, it is evident that the integrant particles muft be fimilar
rhomboids.

If a cryftal can be divided by fections in more directions
than three, what will be the form of the integrant particles?
For example, in the phofphate of lime (the chryfolite of De
L'Ifle) where the fections are parallel to four planes, three of
which have a common interfection. According to what has
been faid above, thefe fections can produce either one hexahe-
dral prifm, or three parallelipipedons, or one triangular prifm.
It is evident that by carrying the divifion, according to thofe
fections, to its greateft length, either the laft hexahedral prifm,
or the laft three parallelipipedons, or the laft triangular prifm,
will be produced. Are thefe laft folids the integrant parti-
cles; are each of them fo; or is there only one of them en-
titled to that denomination ;. and if only one, which of them ?
My anfwer is, only one of them ; and that one, the triangular
prifm, which may be proved thus.

It cannot be denied that the integrant particle is that little
folid which contains the leaft poffible quantity of the body,
without affecting the chemical compofition of the fubftance.
This granted, let us fuppofe the hexahedral prifm to be the in-
tegrant particle. In that fuppofition the laft triangular prifm
muft contain the laft hexahedral prifm, and is equal to the
latter more three little triangular prifms, or in other words to
nine fimilar triangular prifms, while the hexahedral prifm only
contains fix. But the laft triangular prifm and the laft hexa-
hedral prifm each contain an exact proportion, and therefore
a fimilar proportion of chemical component parts; therefore
their difference alfo contain an exact proportion ; but it is im-
poffible to conceive how their differences can contain the ex-
act proportion, unlefs each of the three little triangular prifms
alfo contain it, they muft therefore contain it, and each of
them muft be an integrant particle; therefore the hexahedral
prifm cannot be one ; neither can the parallelipipedons be in-
tegrant particles, as the fame arguments will ftand good againft
them which have been applied to the hexahedral prifm ; there-
fore

fore the triangular prifm muft be the integrant particle :* The forms of the integrant particles, fays the Abbé (vol. I. page 30) may be reduced to three, the tetrahedron or the moft fimple of pyramids, the triangular prifm or the moft fimple of prifms, and the parallelipipedon or the moft fimple of folids, having parallel fides two and two, and as four fides are neceffary to circumfcribe a fpace, it is evident that the above three forms in which the number of fides are fucceffively four, five and fix, are again in this point of view the moft fimple poffible."

The phofphate of lime or chryfolite is a fubftance that has given rife to much curious anecdote. It fhews in what a ftate Abbé Hauy found the mineralogical nomenclature, and points out the accuracy of his analytical method. Achard, a chemift at Berlin had analyfed the chryfolite, and publifhed that it contained, of filex 15 parts, alumina 64, lime 17, and of iron one. This ftartled the celebrated Vauquelin, who had feen Klaproth's analyfis of the chryfolite (the apatite of Werner) containing of lime 55 parts, and of phofphoric acid 45, (probably the water of cryftallization is added to the acid.) A Frenchman of the name of Launoy fent a quantity of this fubftance to Paris, fome of it was purchafed by the *Ecole des Mines*, and Vauquelin was defired to analyfe it. The latter foon fufpected Achard had been mifled by the name, and had not obtained the proper fubftance, a miftake the more eafily made as, fays Vauquelin, " the name of chryfolite was given to a great variety of ftones, fuch as the *peridot*, the *chryfoberil*, the *olivine*, (fince found to be the fame as the *peridot*), and in general to *ftones* having a *yellow colour.*"

He foon difcovered the chryfolite fent from Spain contained lime and phofphoric acid. " I had no fooner made this difcovery, fays he, than I enquired of Abbé Hauy whether he had compared the integrant particles of the chryfolite with thofe of the apatite or cryftallized phofphate of lime. He anfwered me that he had not made the comparifon, but that he would get his papers on primitive forms, (this was four years before the publication of his work) fee what notes he had

* Therefore the fections producing the hexahedral prifm cannot lead to the integrant particle : therefore all fections, though perfectly practicable in cryftals, will not lead to the integrant particle.

made.

Explanation of
the figures and
formation of
cryftals by the
method of Hauy. made on each of thofe fubflances, and immediately compare them together; when with pleafure he found that there was not the leaft variation between them. Thus had the Abbé Hauy difcovered by the help of geometry alone, that which was confirmed by chemical analyfis, and this fatisfactory accord between two fciences apparently fo diftant from each other, while fecuring each others fteps, ferves alfo to fhew the certainty of the principles on which they are grounded."

Journal des Mines, xxxvii. page 21.

A more fingular anecdote is what took place with refpect to the emerald and the beril. Vauquelin had analyfed the emerald of Prerou, and read the refult of it, at a fitting of the *Ecole des Mines,* which is preferved in the Journal, No. XXXVIII. page 96: viz. of filex 64, of alumina 29, oxide of chroma 3, of lime one, and of volatile fubftance 2; I have neglected decimals. Soon after he difcovered a new earth, which he called the Glucine, and gives the following account of it to the National Inflitute: " The Abbé Hauy having obferved a perfect conformity between the ftructure, the hardnefs, and the weight of the beril and the emerald, preffed me a few months back to make an analyfis of thefe two fubftances, to know whether they contained the fame principles, and in fimilar proportions. In the refult, the fact that will moft intereft the Inftitute is the difcovery of a new earth, &c. &c." *Annales de Chimie,* vol. XXVI. p. 157.

I am certain, Sir, that it will give you no lefs pleafure to learn that Vauquelin, in confequence of this difcovery, made an *addition to the paper* read at the *Ecoles des Mines,* beginning thus, " Since the reading of the above paper, having dif-covered a new earth in the beril, and as this ftone, according to the obfervations made by Hauy, contained fubftances fimi-lar to the emerald, I have in that point of view made a new analyfis of this latter ftone." And the former analyfis was immediately corrected, and the 29 parts of alumina became 16 parts of alumina and 13 *of a new earth.* I hope, Sir, it is not too much to fay that, on this occafion a new earth was dif-covered, if not by, at leaft in confequence of, a geometrical analyfis.

But to return to our fubject, the Abbé makes a diftinction between the *integrant particle* and the *primitive form.* The former, as I have faid, is that laft particle, which preferving an exact proportion of the component parts, contains the *leaft number*

number of thofe parts; it is the laft term of mineralogical analyfis. The primitive form, on the contrary, is its firft refult, and retaining the exact proportion of the component parts, contains the *greateft number* of thofe parts. It is eafy to fee that in the cafe above mentioned of the phofphate of lime, the hexahedral prifm will be the primitive form, precifely for the reafons adduced to fhew that it is not the integrant particle. Though the Abbé does not decidedly define the integrant particle as containing the *minimum* of fpace under the *maximum* of furface, and the primitive form as containing the *maximum* of fpace under the *minimum* of furface,* neverthelefs he makes a remark that authorifes the above definitions (which, Sir, you will obferve are mine, left any fault be found with them.) He fays the dodecahedron with rhomboidal fides, which is the primitive form of the garnet (grenat) contains the *maximum* of fpace under the *minimum* of furface; and if it be cut into two equal and fimilar parts, it will prefent the fame form as the bottom of the cell of the honey-comb, which has the fimilar property.

An objection might be taken on the *cuivre pyriteux*, and the *cuivre gris*, or the yellow and grey copper ore of Kirwan, the Abbé mentioning the regular tetrahedron as their primitive form, and not the octahedron as in other cafes. The reafon may be, that all the cryftalline forms of their fubftances which he defcribes are flight modifications of the regular tetrahedron.

" The primitive forms hitherto obferved, fays the Abbé Hauy, are reduced to fix.—The parallelipipedon, the octahedron, the tetrahedron, the regular hexahedral prifm, the dodecahedron bounded by rhombs all equal and fimilar, and the dodecahedron with triangular fides, formed by two right pyramids united bafe to bafe."

He alfo makes a diftinction between integrant particles and *fubftractive particles;* thefe latter are always parallelipipedons. I fhall fpeedily mention whence they derive their name. They are fubftituted for the integrant particles, to facilitate calculations, and it is worthy of obfervation that the parallelipipedon can always be obtained in all diffections of cryftals. Thus far, Sir, I have ftated the firft principles of mineralogical analyfis; I fhall now proceed to the fynthefis.

(To be continued.)

* The inclination of the interfecting planes being the fame.

Defcription

VIII.

*Description of a Filtering Machine invented by Professor Parrot
of Paris. In a Letter from a Correspondent.*

To Mr. NICHOLSON.

SIR,

Filtering ma-
chine by fand in
a reverfed tube.

I ENCLOSE you a drawing of a filtering machine invented
by Profeffor Parrot, of Paris, which for fimplicity and uti-
lity feems fuperior to any other I have met with. You will
perceive that from the curvature of its form, it purifies the
water both by defcent and afcent, and is, confequently, a clofer
imitation of the operations of nature than thofe in which the
water penetrates in but one direction. Among the advantages
which he afcribes to it, he inflances the " prolongation of the
ftratum of fand, which does not confiderably diminifh the pro-
duct of the filtre, but contributes remarkably to the purity of
the fluid," and that " the difference of its water-level has an
effential influence on the quantity of purified water obtained
in a given time;" he therefore recommends an apparatus of
eighteen inches long from A to D, two inches thick, and four
broad, which, he fays, will yield fix (Paris) pints of pure
water every hour: a machine of this fize requires only a dif-
ference of two or three inches in the height of the water.

I am, Sir,

Your conflant reader,

London, Auguft 20, 1804.   A. A.

Defcription of the Machine.

The refervoir G. (*Fig.* 1, *Plate* IV.) may be of any form or
dimenfion which is convenient; the principal part of the ma-
chinery confifting of a fquare veffel bent in the form of an in-
verted fyphon. The curve may be circular, elliptic, or in any
other direction. This veffel is to be filled with fine pure fand
to nearly the height of the dotted line x y, which denotes the
afcent of the water to D, whence it flows into the receiver.
To the part marked A B, which muft always be above this
line, a woollen bag is attached, open at the top and reaching
to the fand: this collects the coarfeft impurities, and prevents
the fand from becoming foul for a longer time. In large ma-

chines

chines a water-tight trap-door may be made at F, for the pur-
pofe of removing the fand when it is overcharged with impu-
rities. The fmall diameter of the machine from which the
drawing was taken, was eight inches from B to E: the per-
pendicular height from C to A B was eight inches and three-
fourths, and from C to D four inches and one-twelfth.

IX.

*Medico-Chemical Refearches on the Virtues and Principles of
Cantharides. By* H. BEAUPOIL.

(Concluded from Page 71 of Vol. VIII.)

HE alfo proved that the black precipitate eafily became dry, *Experiments and*
brittle, and friable in the air; that it reddened the tincture of *obfervations on*
turnfole; that it combined very readily with potafh, difen- *cantharides.*
gaging ammonia; that, when diftilled by an open fire, it
fwelled and yielded an acid liquor, a thick oil, and carbonate
of ammonia; and that it left a dry, fhining, friable coal in the
retort.

Proceeding afterwards to the examination of the yellow mat-
ter remaining in folution in the alcohol, Cit. Beaupoil informs
us, that, when it is concentrated by the evaporation of its
folvent, it retains the fame odour and the fame tafte as the
extract; that it is completely diffolved in water, and reddens
the tincture of turnfole; that it combines entirely with potafh,
without any difengagement of ammonia; and that the refult of
this combination is an homogeneous and glutinous body, foluble
in water and precipitable by a weak acid; finally, that, dif-
tilled by an open fire, it fwells very little, yields an acid liquor,
a black and fetid oil, and carbonate of ammonia; but that, in
general, all thefe products are in fmaller quantity than in thofe
obtained from the black precipitate.

Among thefe different refults the author thought it neceffary
to direct his attention more particularly to the acid, which, as
has been feen, manifefts itfelf fo readily in the infufion of can-
tharides, or in the extract which they afford.

At firft he was of opinion that this acid was analogous to that
of vinegar; he alfo thought that its exiftence might be attri-
buted

buted to the cuftom prevalent with thofe who colle&t the can-
tharides, of expofing them to the vapour of this acid; but when
he found, on fubje&ing fome of thefe infe&s which had been
procured without the affiftance of vinegar to experiment, that
they were fimilar to thofe of commerce, he was obliged to re-
nounce his firft idea, and to endeavour to afcertain the nature
of the acid they offered him, by experiment. It appears that
his endeavours have, in this inftance, been unfuccefsful; for
he finifhes by inferring that his progrefs is not fufficiently ad-
vanced to determine with certainty; and that, although the
acid in queftion has fome analogy with the phofphoric, he, ne-
verthelefs, does not think that it poffeffes all its properties, and,
confequently, is of opinion it fhould be confidered as a peculiar
fpecies, until new experiments have fhewn that to which it
in reality belongs.

The third produ&t of cantharides, called by Thouvenel and
by Cit. Beaupoil, green matter, does not feem to experience
any change from the air, at leaft in its phyfical properties. It
is infoluble in cold water; it liquefies in warm water, floating
on its furface like an oil; alcohol and ether diffolve it, and its
folution in thefe two menftrua is decompofed by water. Oxi-
genated muriatic acid brought into conta&t with this matter,
and renewed from time to time, at firft feems not to have any
a&tion on it, but at length fmall whitifh, brilliant particles are
detached from it, which fall to the bottom of the veffel: in
lefs than a week it lofes its fmell and its colour, and becomes
thick and glutinous; and notwithftanding repeated wafhings, it
conftantly retains the odour of the oxigenated muriatic acid.

Diluted nitric acid, affifted by heat, gives it a ruddy colour,
a rancid, penetrating odour, and alfo a confiderable confif-
tence.

Cauftic foda combines with it without the aid of heat, and
without a difengagement of ammonia. The produ&t of this
union is decompofed by the acids.

Expofed to an elevated heat it fufes, and forms a liquid of an
oily appearance and flightly tranfparent, which, by cooling,
quickly refumes the folid ftate. By a more powerful heat it is
decompofed, its colour changes, a yellowifh oil, very analo-
gous to that obtained from the diftillation of wax, and an acid
phlegm, pafs into the receivers, but not an atom of carbonate
of ammonia.

With

With refpect to the parenchyma forming the refidue of the different macerations, infufions, and decoctions in water, alcohol, and ether, the author, after having afcertained that thefe fluids were incapable of extracting any thing more, treated it with cauftic potafh, which immediately caufed a difengagement of a very fenfible ammoniacal odour. When this odour was diffipated, the liquor was filtered, and inftantly mixed with muriatic acid: the mixture became turbid, and gradually yielded a precipitate, which, dried and thrown on burning coals, exhaled an odour fimilar to that of animal matters in combuftion.

Diftilled in a retort, this parenchyma yielded phlegm, a denfe empyreumatic oil, and a confiderable quantity of carbonate of ammonia. The refidue of the diftillation was a fpecies of coal, from which a white afh was obtained by incineration in the open air, in which were found carbonate of lime, calcareous phofphate, fulphate and muriate of lime, and, finally, oxide of iron.

In recapitulating the quantities of each of the products obtained by means of the experiments which have been cited, the author afferts that one ounce of cantharides, well dried, contains nearly

Black matter	-	1 gros 2 grains;
Yellow matter	-	1 — 2
Green matter	-	1 — 8
Parenchyma	-	4 — 36
Acid	- -	An indeterminate quantity;
Calcareous phofphate		12 grains;
Carbonate of lime	-	2
*Sulphate and muriate of lime		4
Oxide of iron	-	2

To complete the work which Cit. Beaupoil had undertaken, it remained for him to determine the phyfiological properties of cantharides, as well as thofe of the moft effential of their immediate materials, and he appears to have executed this with fuccefs in the fourth part of his differtation. Among other things, it refults from the different experiments which he has made on this fubject,

1ft, That cantharides which have not undergone any preparation, almoft always produce difagreeable effects when

taken

taken internally; hut that thefe effects, with refpect to their intenfity, are proportionate to the age, the ftrength, and the conftitution of the animals, and to the dofe which has been adminiftered to them : That the œfophagus, the ftomach, and the fmaller inteftines, are the parts which are principally affected : that thofe animals which are not overcome by it, experience a defire to vomit, veiy confiderable pains and various affections, which feem plainly to indicate that the parts which have been touched by the cantharides, have a fort of tendency to be diforganized.

2*d*. That the aqueous extract of cantharides, in fmaller dofes than the infects themfelves, produces neaily the fame effects as they do; and alfo, that its action on the urinary paffages is very marked.

3*d*. That the black matter is much lefs active than the extract; that the animals to which it has been given, are only affected by gripings and vomitings, and very rarely are killed by it.

4*th*. That the green matter given internally does not appear to have deleterious qualities, fince all the animals to which even ftrong dofes had been adminiftered, did not feem to be affected uneafily by it.

5*th*. That the yellow matter does not feem to be more active than the green matter.

6*th*. That the extract, the yellow matter, and the black matter, applied feparately to the furface of the body, occafioned vefication in nearly the fame fpace of time.

7*th*. That the green matter, applied externally, does not feem to act when alone; but that its action is fpeedily manifefted when it is divided by wax, and by that means receives the confiftence of a cerate.

I muft not omit to mention, that Cit. Beanpoil was not fatisfied with experiments made on animals, but had the courage to repeat them on himfelf. It was fiom having obtained the information he fought in this manner, that he confidered himfelf intitled to conclude, that the veficating property refides particularly in the extractive part and in the green part of the cantharides, but that the extractive part alone acts on the urinary and genital fyftem.

It will be obvious from the details I have given, that the author has carried the examination of cantharides farther than

Thouvenel.

Thouvenel. But although his work is greatly extended, it neverthelefs is not yet complete, fince much remains to be done, particularly with refpeɛt to the green matter; for it is difficult to conceive how it fhould have no aɛtion on the animal economy when adminiftered internally, fince, applied externally, it produces a veficating effeɛt. This objeɛtion which I have made to the author, the importance of which he acknowledges, will doubtlefs be one of the motives which will determine him to renew his experiments to remove doubts, and to fhew more clearly what is to be expeɛted from the employment of the different parts compofing an ingredient from which medicine has received fuch great benefits.

X.

Experimental Determinations of the Latent Heat of Spermaceti, Bees' Wax, Tin, Bifmuth, Lead, Zinc, and Sulphur. By Mr. WILLIAM IRVINE. Communicated by the Author.

Bedford Street, Aug. 24, 1804.

It will fcarcely be denied that the difcovery of the exiftence of latent heat in all fluid and vaporous bodies, is one of the moft curious and important hitherto developed in the progrefs of chemical philofophy. The merit of firft inveftigating this fubjeɛt is univerfally attributed to the celebrated Black. By a few fimple and clear experiments he demonftrated, that, before any portion of ice can become water, it muft receive or abforb as much heat as would have raifed the temperature of an equal quantity of water by 140°. By other experiments, in fome of which Dr. Black was affifted by my father and Mr. Watt, it was proved in a manner equally fatisfaɛtory, that water cannot be converted into fteam unlefs it admit a quantity of heat fufficient to have heated the water 8 or 900°. Having proceeded fo far by experiment, Dr. Black made a general inference, and extended his theory to all cafes of fufion and vaporization whatever.

The only other philofopher, as far as I know, who has attempted to determine the exaɛt quantity of the latent heat in other bodies befides water, was Dr. Irvine. Landriani made fome

fome experiments to prove that the fluidity of alum, fulphur, and fome metals, was accompanied with an abforption of latent heat ; but I believe he made no attempts to afcertain the precife quantity. In Dr. Black's lectures we are informed, that Dr. Irvine found the latent heat of fpermaceti to be 145°, of bees' wax to be 175°, and of tin to be 500°. From the very imperfect notes which I poffefs of the methods ufed to determine the two former, I believe that the 145° are meafured by the capacity of fluid fpermaceti, and the 175° by that of fluid wax: But of this fubject I will take another opportunity to treat more amply. I am perfectly ignorant of every circumftance regarding the latent heat of tin as determined by my father, having been unable to find any notes of his experiments for determining this point.

Experiments
made by the
author.

During the courfe of the prefent year I have turned my attention a little to this fubject, I mean to the inveftigation of the quantity of latent heat neceffary for the fufion of various bodies. I was perfuaded that an addition to our knowledge of latent heat would at leaft increafe the ftore of facts, and might perhaps give rife to fome improvement or correction of theory.

The veffels.

The veffels which I employed in all my experiments were Florence flafks, of which the neck was cut off. In thefe the water made ufe of was contained, and the veffel was fupported on a flight wooden ftand, which prefented a very fmall furface to abftract heat from the materials. The orifice at the fuperior part of the veffel, was in general not more than fufficient to permit the ready introduction of the fluid examined; probably from an inch and a quarter in diameter to a little more, fo that a very fmall furface of the water was expofed to the air. The weight of the glafs was in all cafes previoufly afcertained. Thefe circumftances being premifed, I proceeded as follows:

Determination
of the latent heat
of fluid bifmuth
(or the number
of degrees
through which
any quantity of
bifmuth would,
by the heat it
g ves out in con-
gelation, raife an
equal quantity of
b fmuth, from
beneath the
fr zing point of
the metal).

The firft fubftance which I fubmitted to examination was bifmuth. The melting point of this metal was, by the thermometer which I ufed, 480°. This is fo near the point found by my father, to wit 476°, that I fhall confider his determination as correct, as he combined and compared the different ways of computation and obfervation, and I know the thermometer which he ufed to have been made with confiderable care. Into a glafs veffel which weighed 411 grains, I put 2236 grains of water, of the temperature of 62°. I then removed from the fire a quantity of fluid bifmuth. I waited till

it

it was partially folid, at which time I inferred that both the folid and fluid metal were of the temperature of 476°. I next poured a portion of the fluid bifmuth into the water. In one minute the thermometer ftood at 86°; in two minutes at 85¾: The true temperature at the moment of mixture was therefore 86¼, fuppofing equal temperatures to be loft in equal intervals. There was alfo a quantity of fteam formed. I weighed the veffel with its contents, and found that it had gained 1589 grains. This therefore fhould be the quantity of bifmuth poured in, if there had been no lofs. But on drying and weighing the metal, it appeared to amount only to 1555 grains, and 34 grains confequently were loft. The bifmuth was cooled 389¼°, the water was heated 24¼°. Then 1555 grains of water would have gained 34°. Thefe 34°, meafured by the fpecific heat of bifmuth, as ftated in Thomfon's Syftem of Chemiftry at .042, are equal to 810°.9: But the bifmuth, after becoming folid, loft 389°.75, which being fubtracted from 810.9, there remains 421°.15, which cannot be account-ed for by the cooling of the folid bifmuth, and muft therefore be the whole or a part of the latent heat of the fufed metal.

Bifmuth, on the point of congealing, was poured into water, the quantities, temperatures, &c. being known; and the common temperature was taken.

Inference of the latent heat.

But the latent heat muft be greater than this, for 411 grains of glafs were alfo heated 24°.25. If the capacity of this glafs be taken at .174, as Kirwan found flint glafs to have, and I have found green bottle glafs to have a capacity of .173 by feveral experiments, whence it is probable that the glafs of Florence flafks has its capacity not very wide of thefe numbers; if then .174 be taken as the capacity of this glafs, thefe 24°.25, which the bifmuth communicated to the 411 grains of glafs in the veffel, are, when meafured by the capacity of bifmuth, equal to 96°.4: And 1555 grains of glafs would have gained 25°.4, which muft be added to 421°.15 already found, and makes 446°.55 for the latent heat of bifmuth.

Correction, for the veffel;

But this is obvioufly ftill too little; for, as has been already mentioned, there was a good deal of fteam formed. The amount of the heat thus loft is extremely difficult to affign. I fhall, however, make an attempt to guefs at, rather than determine it. All the 34 grains muft not be reckoned to have been loft by evaporation. In fpite of all my efforts, I could perceive that, fome, though certainly a fmall quantity of bifmuth went off with the water, in form of a number of fmall particles floating in the liquid: and perhaps alfo a fmall por-tion

and for fteam produced.

tion of water might be wasted during the procefs. If we allow
a half of all the lofs to be accounted for in thefe ways, there
remain fixteen grains of water which have been converted
into fleam.   The latent heat of fleam was computed, by Mr.
Watt, to be equal to 940° : But this fleam cannot be allowed
fo much heat as this; and though it may be difficult to point
the quantity to be fairly granted, yet I fhall expect to be with-
in bounds when I eftimate it at one half of 940°, or 470°.   In
this cafe we fay, 16 grains of water have been heated 470°;
fixteen grains of bifmuth would be heated by the fame quan-
tity of caloric, 11190°.4; and 1555 grains + 16 grains fup-
pofed to have gone off with the water, = 1571 grains of bif-
muth, would be heated 113°.9.   This quantity of heat, there-
fore, ought to be added to the 446°.55 already found, and
would amount, in all, to 560.45 latent heat of bifmuth.

Latent heat of
bifmuth deduced
560¼ degrees.

Other experi-
ments with bif-
muth.

I am fenfible that there are feveral gratuitous fuppofitions in
this laft part of the reafoning, and I do not therefore lay much
ftrefs on it.   I endeavoured to confirm or refuté the truth of
the inductions, by making an experiment exactly on the fame
principles, but where, by dexterity, I might prevent the for-
mation of fleam wholly or in part.   In one inftance I fucceeded
tolerably well, and then the latent heat, with every correc-
tion, amounted to nearly 600°.

I repeated thefe experiments for determining the latent heat
of bifmuth many times, and the refult is expreffed in the
following

T A B L E :

Tabulated.

No. of Ex- periments.	Latent Heat by firft Computa- tion.	Correction for the Heat re- ceived by Glafs.	Whole Latent Heat.
1	457	23	480
2	411	29	440
3	412	28	440
4	465	33	498
5	480	29	509
6	438	27	465
7	465	30	495
Mean	446.8		475

On the whole, therefore, it appears that we shall not exceed the truth when we estimate the latent heat of bismuth at 550°.

I made, in like manner, two experiments to ascertain the latent heat of tin, and of these the results were—

Mean result 550° for the latent heat of bismuth.

Experiments on tin.

Latent heat 507°

Expt.		
1	520	
		Mean, 507°.
2	495	

This agrees remarkably well with the determination of this point, said to have been made by my father, in Black's Lectures.

Latent heat of zinc 493°.

Zinc is computed, by Bergman, to melt at 700° of Fahrenheit's thermometer. Taking that for granted, I made three experiments on the latent heat of zinc, in a similar way with that already related with bismuth. The results were as follow:

Experiment.	Latent Heat by first Computation.	Corrected.	Whole L. H.
1	490	28	518
2	476	22	498
3	443	32	475
Mean	469		493

In these experiments no allowance is made for loss by steam, which, however, by the dexterity acquired by practice, I was enabled to render very small.

Latent heat of lead 146°

Lead I found to melt at a point above 584°. Owing to the shortness of the thermometric scale, I could observe no higher. I suppose therefore that 594°, as found by Dr. Irvine, cannot be materially distant from the truth. Assuming it as true, I proceeded to make several experiments on the latent heat of lead. In doing this I was led immediately to notice, that melted lead does not by any means produce so much steam as other metals do when poured into water, even under the most careless management; and of this the reason will appear from the following table :

Experiments on the Latent Heat of Lead.

No.	Latent Heat.	Corrected.	Whole Latent Heat.
1	127.8	$10^\circ.3$	$138^\circ.1$
2	142.8	$11^\circ.$	$153^\circ.8$
3	149.9		
4	161.8		
5	131.5		
Mean	142.7	Mean	$145^\circ.9$

In thefe experiments I have, by accident, loft the notes of the determination of the weight of the veffel employed in all but the two firft inftances: notwithftanding which it may be fairly inferred, that the mean latent heat of lead is about 150°, a quantity certainly unexpectedly fmall, and which, in many more experiments than thefe related, I was at pains to examine, without being able to difcover any material inaccuracy. In the fecond experiment, where a little fteam was formed, I afcertained the lofs to be four grains. If thefe four grains be fuppofed, as in the cafe of bifmuth, to contain 470° of heat, the computed addition to the latent heat of lead will be $20^\circ.3$, making in all a little more than 162°. This is certainly a very peculiar and unexpected quality of this metal.

Experiments on fulphur.

The only other fubftance which I have fubmitted to examination for the purpofe of afcertaining its caloric of fluidity, is fulphur. The melting point of fulphur is commonly ftated, in elementary works, to be at 212° : But that this is not accurate any one may convince himfelf, by immerfing a quantity of fulphur in boiling water, where it remains altogether unaltered. By every trial which I have been able to make, I am convinced that the fufion of fulphur takes place about the temperature of 226°. I fay about, becaufe the communication of heat among the particles of fulphur is very flow, and the thermometer is often encrufted with folid fulphur, which, fome how or other, certainly cools below the liquid in which it is immerfed. In experiments for afcertaining this point, the thermometer ought to be kept in conftant motion. The refults of my experiments for finding the latent heat of fulphur, are ftated in the following table.

Experiments

Experiments to afcertain the Latent Heat of Sulphur.

No.	Latent Heat.	Corrected.	Whole Latent Heat.
1	144°.56	8°.	152°.56
2	131°	7°.	138°.
3	140°	4°.5	140°. 5
4	136°		
Mean	137°.89	Mean	143°.68

Latent heat of fulphur 143½°.

In thefe experiments I have fuppofed, from experiments of my own, the capacity of fulphur to be .189, which does not materially differ from Mr. Kirwan's determination. In the other cafes I have trufted chiefly to the numbers given in Thomfon's Chemiftry, though thefe are not always wholly unexceptionable. In every inftance I have fuppofed the femi-liquid to have the temperature of the melting point, which I believe is generally true; but fome practice is required to feize the moment before the frozen particles float in the fluid fub-ftance. In experiments on fulphur efpecially, inattention to this circumftance caufes very great inaccuracy, and was the caufe of confiderable embarraffment to me before I obferved my error.

A comparative table follows of the caloric of fluidity of all fubftances hitherto examined :

Subftance.	Melting Point.	Latent Heat.	Ditto in Degrees meafured by Capacity of Water.
Ice	32°	155°.555	140°
Spermaceti	113°	145°	
Bees' Wax	142°	175°	
Tin	442°	500°	33°
Bifmuth	476°	550°	23°.65
Lead	594°	162°	5°.604
Zinc	700°	493°	48°.3
Sulphur	226°	143°.68	27°.145

Table of latent heats.

In all thefe inftances the latent heat is expreffed in degrees meafured by the capacity of the relative folid, excepting in the cafes of fpermaceti and bees' wax, which are in degrees meafured by the capacity of the fluid. I endeavoured to rec-

General remarks.

tify

tify this fo as to make the comparifon more fair, by determining the fpecific heat of folid wax and fpermaceti; but I have not been able to fatisfy myfelf with either of thefe points, owing to the foftnefs and confequent abforption of latent heat, which a very low degree of heat induces in both thefe bodies. The numbers expreffing their latent heats are therefore too low. On infpection of the above table, there does not appear any ratio by which the quantity of the caloric of fluidity feems to be guided: it certainly does not increafe with the difficulty of fufion, but moft probably has fome connection with the relative capacity of each body in its folid and fluid ftate. The determination, however, of the capacity of any of the metals in a fluid form, excepting mercury, muft be regarded as an extremely difficult tafk.

XI.

Strictures on Mr. Dalton's *Doctrine of Mixed Gafes, and an Anfwer to Mr.* Henry's *Defence of the fame. In a letter from Mr.* John Gough.

To Mr. NICHOLSON.

SIR,

Caufes of thefe ftrictures.

I Have ventured to defend the chemical union of water and air, as well as the homogenity of the atmofpherical gas. My thoughts on thefe fubjects are briefly ftated in your Journal *; and the farther profecution of the enquiry compels me to make an open attack on my friend Mr. Dalton, and his new convert, Mr. Henry. The difpute fhall be fairly conducted on my part; that is, it fhall confift of arguments which I am ready to abandon as foon as they are refuted; this promife is due to friendfhip, as well as the obligations of truth.

Dalton's a hypothefis not a theory.

The firft thing to be afcertained is the proper appellation of Mr. Dalton's opinions. His doctrine of mixed gafes is offered to the public as a mechanical theory, founded on chemical facts: a little attention, however, to Mr. Dalton's effays, will deprive it of all claim to the title of a theory. This is evident from the nature of the mechanical philofophy; every branch

* Vol. VIII. Page 243.

of which admits of a mathematical demonftration, derived from Newton's definitions and laws of motion : but Mr. Dalton has not attempted to give ftability to his new ideas, by the aid of the mathematics ; on which account the only appellation due to his doctrine, is that of a hypothefis.

No philofopher ought to difregard the means which are *The hypothefis* able to confirm his opinions, becaufe thefe means may be found, *not mechanical.* upon trial, to fubvert them. This I believe to be the cafe with my friend's hypothefis ; for I have endeavoured to fhew the fallacy of it by mathematical arguments. The refult of this attempt will in all probability appear in the next volume of the Manchefter Memoirs ; when the merits of the effay will be determined by geometricians, who are the proper judges of fuch productions. This want of geometrical demonftration efcapes the notice of the chemift, becaufe my friend has feemingly fupplied the deficiency by a number of probabilities of an experimental nature ; but it is almoft fuperfluous to remind either him or your readers, that a myriad of fuch proofs cannot uphold a doctrine which is repugnant to the mechanical philofophy.

The two leading maxims which are derived from thefe proba- *Conclufions* bilities, and form the bafis of the hypothefis, are thus briefly *from thefe pro-* exprefled by Mr. Henry : " mixed gafes neither attract nor repel *babilities.* each other, and every gas is as a vacuum to every other gas*". Mr. Dalton, reafoning from thefe premifes, furrounds our globe with an independent atmofphere of vapour, the preffure of which preferves all the water on the earth's furface in a liquid ftate, and prevents the ocean itfelf from efcaping through the air, by virtue of its own elaftic force ; at leaft the laft is a fair inference from his own conclufions.

Although I have demonftrated the exiftence of an atmo- *Dalton's reafon-* fphere of vapour to be a mechanical impoffibility, in the effay *ing incomplete.* mentioned above, the reality of the thing fhall be fuppofed at prefent, for the purpofe of detecting the fallacy of the doctrine in the fundamental maxims of it. The truth is, Mr. Dalton has difcontinued his train of reafoning too foon ; for, had his arguments been purfued to their proper limits, they would have difcovered the incompatibility of the hypothefis and natural appearances. This omiffion is eafily fupplied, and will be as eafily comprehended, by a perfon who underftands the laws of hydroftatics.

* Phil. Journ. Vol. VIII. Page 298.

If

A fecond particle may follow a former.

Air preffes upon water.

Air a perforated piflon, by the hypothefis.

Air not a vacuum to vapour.

Impenetrability no obflacle to chemical union.

One gas not a vacuum to another.

If a particle of vapour can pafs freely through the air, a fecond can alfo fucceed it at any given diftance; becaufe the latter may undoubtedly purfue the track which the former has already traverfed: confequently, a feries of fuch particles, poffeffing the denfity of water, might be raifed into each perpendicular pore of the atmofphere, by the application of a proper force to the furface of a collection of water fupporting fuch an atmofphere. Now the exiftence of fuch a force is certain; becaufe when air is injected into either leg of an inverted fyphon containing water, it conftantly difturbs the equilibrium of this fluid; that is, the gas preffes upon the liquid, notwithftanding the fuppofition, that the former is a vacuum to the particles conftituting the latter. If now we combine the poftulates of the hypothefis with the preceding fact, a column of air, which occupies the upper part of a veffel containing water in its bottom, becomes a heavy pifton, having its fubftance perforated in every direction by pores of eafy tranfmiffion, which are, at the fame time, feparated by partitions impervious to water. After contemplating this imaginary ftructure of the atmofpherical gas, let the reader inveftigate the confequences of it, and compare them with natural events. The impenetrable parts of the gafeous pifton would compel the water to afcend along its perpendicular pores; in which it would be kept duly condenfed by the preffure of the incumbent vapour. The fpecific gravity of this column, compounded of air and water, would exceed that of the external air; confequently the upper extremity of it would conftantly glide over the edges of the veffel into the atmofphere. Thus there would happen a double lofs of water, namely, by evaporation and percolation: but nothing of the kind is obfervable in nature; confequently the air is impenetrable to the conftituent particles of water not heated to the boiling point.

The circumftance of water not being able to penetrate air at low temperatures, is no obftacle to the chemical union of the two fubftances; for many aqueous folutions of falts occupy lefs fpace, when completed, than the materials formerly did of which they are compofed. This fact has been eftablifhed by the prefent Bifhop of Llandaff; and it proves that bodies, which are mutually impenetrable, may be fufceptible of the bond of chemical affinity.

After

After all that has been said about the conftitution of gafes, the notion is incorrect, ftrictly fpeaking, which fuppofes each gas to be as a vacuum to every other. The foregoing obferva- tion may alfo be extended to mixtures of carbonic acid gas with water; and the following inftances may be adduced in confirmation of the propofition : When a particular gas is de- veloped in a veffel containing common air, the firft portion that comes over has in it lefs of the fpecific gas than the fe- cond; nor is the fecond equal to the third in purity. This circumftance fhews, that the new-formed gas does not find a vacuum in the air of the veffel; on the contrary, the two fluids produce a mechanical mixture, which is gradually ex- pelled, until no part of the air remains in the tube or bottle. In like manner, if a bottle of highly aerated water be opened fuddenly, the rapid expanfion of the liberated gas ejects a great part of the contents, thereby proving that water oppofes an obftacle to the dilatation of this gas. In reality, Mr. Dalton allows one gas to be an impediment to the motions of another; but at the fame time he maintains, that two fuch fluids finally overcome their mutual obftructions, and occupy the fame fpace in a ftate of perfect independence. This is a propofi- tion which may be juftly fufpected of being a folecifm in pneu- matics, until the author of it has proved the contrary by a ri- gorous demonftration.

Proved by expe- riment.

Amongft the many probabilities which have been offered in fupport of the hypothefis, perhaps none are more ingenious than the remarks of Mr. Henry ; and the part which I have taken in the prefent letter, obliges me to place them in a new light. This attempt muft, however, be preceded by a theory, which will explain the relations of thofe gafes that neither attract nor repel each other : fuch an explanation became a neceffary part of pneumatics, from the time when Dr. Prieftley made his experiments upon mixtures of this defcription. The following, then, is a fketch of a theory, having for its founda- tions the mutual repulfion of homogeneous particles, and the reciprocal refiftance which gafes have been fhewn to poffefs.

A new theory of mixed gafes ne- ceffary.

When two fuch gafes come into contact, parcels of each will be detached by every flight force, and enveloped in the fubftance of the other. In this manner, the two fluids will be broken to pieces, and blended in one mafs forming a mecha-

Sketch of the theory.

nical

nical mixture; the component parts of which cannot be separated without the intervention of chemical agents, becaufe the disjoined fragments of each gas will be prevented from re-

uniting by their mutual repulfion. There is one exception, however, to the general rule; for when a liquid is found in a mixture, the component parts will follow the law of their fpecific gravities; becaufe an inelaftic fluid is not actuated by an intrinfic repulfion, in all other cafes the gafeous fragments will continue to be farther and farther fubdivided, and will conftantly affume a new arrangement from the flighest agitations. The conftituent parts of fuch a mixture exert their force in perfect union; and this circumftance diftinguifhes it from Mr. Dalton's compound, the conftituent gafes of which prefs feparately upon all furfaces.

The neceffary premifes being now fettled, it is time to try the powers of the theory upon Mr. Henry's experiments. If, then, ten meafures of water, containing an equal bulk of carbonic acid gas, be preffed by a column of the fame gas, equivalent in force to 30 inches of mercury, the ftate of the aqueous compound will remain invariable; becaufe the fpring of the gas in the water appears, by Mr. Henry's experiments, to be equal to the fpring of the incumbent gas: therefore, fhould a gafeous particle happen to efcape from the compound, an equal particle, from the upper part of the veffel, will replace it immediately. But if the incumbent carbonic acid be made to give place to a mafs of common air of the fame elaftic force, the furface of the aqueous compound will undergo no change of preffure; but the gafeous part of it, meeting with no repulfion from the column of air, will begin to form a mechanical mixture with it, according to the theory. The parcels of the carbonic acid will alfo remain diftinct, according to the fame, after their efcape from the water; and as a number of them will be arranged on the furface dividing the two mediums, they will form fo many obftacles to the difcharge of their kindred gas; the egrefs of which will be confined to the intermediate compartments of eafier tranfmiffion confifting of common air. The divifion of the furface between the two mediums, into compartments of eafy tranfmiffion and impenetrable points, ultimately produces a fort of equilibrium, which affigns their refpective portions of the elaftic acid, to the gafeous

bus compounds. The origin of this equilibrium is eafily collected from the preceding theory and the laws of pneumatics. For the globules of carbonic acid, contained in the gafeous compound, invariably exert the force of 30 inches of mercury; they therefore prove too powerful for the rarefied gas of the water, which they comprefs, and infinuate part of their own fubftances into the fluid. Whilft this operation is going on at innumerable points in the furface, portions of the fame gas are conftantly forced through the compartments of eafy tranf-miffion, by the flighteft agitations. Now the quantity of gas which is difcharged into the air of the jar, is greateft at firft, and continually diminifhes; on the contrary, the quantity that is forced into the water through the former paffages, is leaft at firft, and increafes afterwards; confequently the two quantities ultimately become equal, and eftablifh the equilibrium in queftion, by the contrariety of their effects.

The preceding is a general propofition, which explains a variety of appearances, fuch as Mr. Henry has defcribed, by by the well known principles of mechanics. Should the confideration of it be attended with conviction on his part, he perhaps will give a new turn to his experimental enquiries, and endeavour to difcover the law of affinity, which connects water with the different gafes. It is almoft certain, that this law is not the fame in all cafes, as will be feen by comparing the experiments of M. la Sauffure on the folution of water in common air, with Mr. Kirwan's obfervations on the folvent powers of hidrogen; which may be found at page 14 of the firft edition of his Effay on Phlogifton. *(Concluding remarks.)*

JOHN GOUGH.

Middlefhaw, Aug. 23, 1804.

P. S. The following error requires correction: In Vol. VIII. page 244, line 21, before the words, *as oft as water,* read *therefore,* and ftrike out the fame conjunction in line 22.

XII.

An Enquiry concerning the Nature of Heat and the Modes of its Communication. By BENJAMIN COUNT *of Rumford,* V. P. R. S. &c. *Abridged from the Philofophical Tranf-actions for the Year* 1804.

Experiments on heat.

AFTER remarking that all difcoveries on an agent of fuch extended operation as heat cannot fail to be eminently ufeful, the author proceeds to defcribe the apparatus ufed in the experiments now to be defcribed: They were the following.

Inftruments. Thermometers.

1. Mercurial thermometers carefully conftructed, having cylindrical bulbs, four inches long and four tenths of an inch in diameter, and their tubes from 15 to 16 inches long; the air being excluded, and the graduations according to Fahrenheit, exhibiting eight parts of degrees by means of a nonius.

Inftruments. Cylinders to contain hot water, and fuffered to cool with different kinds of clothing.

2. Four cylindrical veffels of thin fheet brafs, for afcertaining the warmth of clothing. *Fig.* 1. *Plate* I. The veffel is clofed at both ends; but has a neck at the top, into which hot water is occafionally poured, and in which one of the thermometers is fitted and placed during the time of an experiment, fo that its long bulb fhall occupy the axis of the veffel, and will fhew its mean temperature. Another cylindrical neck proceeds from the lower furface, and is fitted upon the adjuftable part of the wooden ftem beneath. The veffels are four inches long and four inches diameter, and the necks are about eight tenths of an inch in diameter, the upper one being four inches long and the lower three inches. When the veffel is clothed and charged with hot water, its rate of cooling by expofure to the quiet air of a large room,* will fhew the relative warmth of each particular kind of clothing.

In fome of the experiments the ends of the inftrument were permanently covered by the application of a thin wooden box to each, the box being varnifhed and covered with fine writing paper, and filled with fine eider down, and a cap of fur was pulled over the box, and the projecting neck. The clothing of thefe cafes was applied for experiments to the cylindrical furface.

Two of the inftruments (No. 1 and 2.) were thus covered up at the ends, and the other two (No. 3 and 4) were left in the ftate reprefented *Fig.* 1, without the permanent coverings.

In

In each experiment two fimilar inftruments (fuppofe No. 1 and No. 2, or No. 3 and No. 4) were ufed, one *naked* and the other *covered*; fo that in each experiment the naked inftrument ferved as the ftandard of comparifon with the other.

The experiments were made and regiftered in the following manner: the two inftruments ufed in the experiment, placed over their wooden ftands, being fet down on the floor, were filled to within about 1½ inch of the tops of their cylindrical necks with boiling hot water; and a thermometer being put into each of them, they were placed at the diftance of three feet from each other, on a large table in a corner of a large quiet room, 24 feet long, 19 feet wide and 13 feet high, where they were fuffered to cool undifturbed. Near them, on the fame table, and at the fame height above the table, there was placed another thermometer, fufpended in the air to the arm of a ftand, to afcertain the temperature of the air.

Every caution was ufed to prevent difturbance by currents or agitation of the air, whether by partial heat or the intrufion of any perfon during the progrefs of any experiment.

By the refults of a great number of experiments, it was found that the fame inftrument cooled through any given (fmall) number of degrees, for inftance 10°, in very nearly the fame time, whatever was the temperature of the air of the room; provided always that the point from which thefe 10 de- grees commenced, was at fome conftant number of degrees above the temperature of the air at the time being. The in- terval chofen by the Count lay between the 50th and the 40th degrees above the temperature of the air in which the inftru- ment was expofed to cool; when for inftance, the air was at 58°, the interval commenced at 108, and ended at 98°.— When the air was at 64½° it commenced at 114½° and ended at 104½°.

The warmth of any covering, or its power to confine heat, was eftimated by the time employed in cooling through that interval.

As it fometimes happened, though very feldom, in the courfe of an experiment (which commonly lafted feveral hours) that the moment of the paffage of the mercury through one or both of thefe extreme points was not obferved, it was of im- portance to determine the fame by interpolation from the other points obferved. To do this, the author availed himfelf of

the

the following law of cooling of hot bodies in a fluid, which he found by experiment to be applicable without fenfible error in the prefent cafe: It is that, if the equal portions of a right line reprefent fucceffive intervals of time, and perpendiculars be erected upon the fame, to denote by their lengths the degrees of the excefs of temperature of the hot body beyond that of the cold medium, at points denoting the correfponding inftants of time, the line joining the extremities of the perpendiculars will be the logarithmic curve. Whence if two temperatures and the elapfed time be obferved, it will be eafy by the help of a table of logarithms to determine the time at which any intermediate temperature took place. This is exemplied by the Count, who then proceeds to relate his experiments.

Exp. 1.
A clothing of Irifh linen caufes the heat to efcape much quicker than from a bright metallic furface.

Exp. 1. One of the veffels, No. 1. having its ends clothed as before defcribed, and its polifhed fides naked, was filled with hot water. Another veffel, No. 2. alike in all refpects, but having its fides clofely clothed with Irifh linen, fuch as is fold in London at 4s. per yard, was filled in like manner, and both were fimilarly expofed to cool.

The naked inftrument employed 55 minutes in cooling, from 94° to 84° Fahrenheit (the air of the room being at 45°.)—But the clothed inftrument was cooled through the fame interval in 36½ minutes, confequently this clothing expedited the emiffion of heat inftead of confining it.

When both inftruments were cooled to 42° they were removed into a warmer room at 62°, and the clothed inftrument was alfo found to acquire heat confiderably fafter than the other.

Whether the linen accelerated the cooling by affifting the fucceffion of frefh particles of air, or by promoting the efcape of heat by radiation, were points to be determined.

Exp. 2. The fame effect from a coating of glue.

Exp. 2. To decide this, the linen of No. 2. was removed, and the fides were thinly coated with glue. In thefe circumftances, while the ftandard or naked inftrument cooled through the interval in 55 minutes, the coated inftrument employed only 43¼ minutes.

In reafoning upon this experiment, the author concluded, that if the glue operated only by preventing the air from attaching and fixing itfelf to the polifhed metallic furface, and confequently in that manner facilitated its circulation and the cooling; it would be of no confequence, whether the furface

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was

was covered by one or more coatings of glue: But, on the contrary, if the radiations of heat were facilitated and increafed, it might be expected that a greater effect would be produced by two coatings than by one.

Exp. 3. The experiment was therefore tried, and it was found that the inftrument ufed employed only 37½ minutes in paffing through the interval.

Exp. 3. More coatings of glue increafed the effect.

Exp. 4, 5, 6, 7. When the experiment was repeated with clear colourlefs fpirit varnifh, the fame effect was produced, and it was augmented as far as by four coatings. But on proceeding as far as eight coatings, the limit of the greateft effect was found to have been paffed.

Exp. 4, 5, 6, 7. Alfo fpirit varnifh,

Exp. 8, 9, 10. Black paint (lamp black and fize) upon the varnifh increafed the cooling effect a little. When the inftrument was cleaned and then painted, its rapidity of cooling was nearly the fame = 35 minutes. And with white paint the difference was not confiderable, as the effect was produced in 36 minutes. As the paint was laid on in feveral fucceffive coatings, the Count remarks that little dependence is to be placed on thefe refults as indicating a difference from colour.

Exp. 8, 9, 10. —and black fize paint,

—and white.

Exp. 11. The clean inftrument being fmoked black over a wax candle, was found to have cooled through the interval in 36⅛ minutes, while the ftandard employed 55½ minutes. The lamp black, when wiped off and weighed, amounted to lefs than $\frac{1}{15}$ of a grain, though it had completely covered 50 fquare inches.

Exp. 11. And of fmoke from a candle.

With a view to a more accurate determination of the velocity of cooling, it was neceffary to afcertain what heat efcaped through the permanent clothing at the ends. This was done by obferving the times of cooling with and without the clothing, and comparing the effects with the refpective furfaces of expofure. For the whole furface of the inftrument 85.195 fquare inches, is to the furface of its vertical fides; fo is the whole quantity of heat paffed off = 10000 to the quantity that paffed through the fame fides = 5885. And fince by obfervation the naked inftrument required 45½ minutes to cool through the fame interval as was paffed through in 55½ minutes when the ends were covered, the author concludes that 45½ : 55½ :: 5885 : 7015 = what would have paffed in the latter interval through the upright furface, and confequently that the remainder of the heat = 2985 parts muft have paffed through the covered parts of the inftrument.

Determination of the efcape of heat at the flat ends of the inftruments.

By

By applying thefe refults to the numbers in *Exp.* 11. where the cooling was effected in $36\frac{1}{2}$ minutes, the Count fays as $55\frac{1}{2}$ minutes give 2985 heat paffed through the covered ends, fo will $36\frac{1}{2}$ minutes give 1942 parts. And this taken from 10000, the whole heat loft, will leave 8058 for the heat that really paffed through the upright fides. But it was found that 7015

pafs through the naked fides in $55\frac{1}{2}$ minutes : Whence $7015 : 55\frac{1}{2} : : 8058 : 63\frac{1}{4}$. And confequently, the corrected times are $36\frac{1}{2}$ and $63\frac{1}{4}$, which exprefs the velocities of the paffage of heat through the furface of the naked metal, and that which was blackened with fmoke, viz. as 5654 to 10000 nearly.

In the fame manner the velocities of the paffage of heat in the experiment No. 6. are fhewn to have been as 4566 to 10000.

A new courfe of experiments. It has been remarked that, in thefe curious inftances of the effects of modification of furface or clothing upon the tranfitions of heat, the effect may have been favoured by communication to the air, or by facilitating the procefs of radiation. The author's reafoning upon *Exp.* 2. appeared not fo decifive as to need no fupport from experiments of a different clafs. He therefore conftructed an inftrument for meafuring the effects of radiation, which is feen in *Fig.* 2, *Plate* I.

Inftruments for improving radiant heat. Like the hygrometer of Mr. Leflie,[*] (as the Count obferves) it confifts of two glafs balls at the ends of a tube C and E. The tube is of fuch a diameter that one inch in length would contain 15 grains of mercury; the balls are 1.625 inches in diameter; the upright ends of the tube C and E are each 10 inches long; the horizontal part D is 17 inches ; and the board A B, to which it is attached, is 27 inches long, 9 inches wide, and one inch thick. The pillar F fupports a circular vertical fcreen made of paffeboard, covered with gilt paper on both fides, the ufe of which is to protect one of the balls from rays intended only to act upon the other. The balls contain only air, and a fmall drop of coloured fpirit of wine is introduced by means of a fhort tube projecting from one of the elbows; which fhort tube is then hermetically fealed. By a little management the bubble of fpirit is brought to reft in the middle of the horizontal tube ; and when the temperature of

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[*] Philofophical Journal, quarto feries, III. 461.

the

the air in either of the balls is made to exceed that of the other, the increafed elafticity caufes the bubble to move towards the colder ball.

By a fimple contrivance of fliding boards, the hot bodies were moved by rack-work and a winch to any diftance from the thermofcope without the attention of the obferver being taken off from the bubble in the tube, and the diftances were alfo fhewn by a graduated fcale and nonius. *Fig.* 3. *Plate* I. fhews one of thefe bodies. It is a metallic cylinder having its bafe,|which is to be prefented to the thermofcope, vertical, and its neck obliquely placed for the purpofe of introducing the hot Water, and alfo a thermometer for fhewing its temperature at any required time.

The experiments and obfervations which conftitute the remainder of this memoir will be given in our next.

XIII.

Experiments and Calculations relative to phyfical Optics. By THOMAS YOUNG, *M. D. F. R. S. From the Philofophical Tranfuctions for* 1804.

I. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

IN making fome experiments on the fringes of colours accompanying fhadows, I have found fo fimple and fo demonftrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to eftablifh, that I think it right to lay before the Royal Society, a fhort ftatement of the facts which appear to me fo decifive. The propofition on which I mean to infift at prefent, is fimply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the moft prejudiced, that the affertion is proved by the experiments I am about to relate, which may be repeated with great eafe, whenever the fun fhines, and without any other apparatus than is at hand to every one.

Exper. 1. I made a fmall hole in a window-fhutter, and covered it with a piece of thick paper, which I perforated with a fine

General laws of the interference of light proved in the production of fringes of colours.

Exp. 1. Fringes bordering the fhadow of a flip of card are prop

a fine needle. For greater convenience of obfervation, I placed a fmall looking glafs without the window-fhutter, in fuch a pofition as to reflect the fun's light, in a direction nearly horizontal, upon the oppofite wall, and to caufe the cone of diverging light to pafs over a table, on which were feveral little fcreens of card-paper. I brought into the fun-beam a flip of card, about one-thirtieth of an inch in breadth, and obferved its fhadow, either on the wall, or on other cards held at different diftances. Befides the fringes of colours on each fide of the fhadow, the fhadow itfelf was divided by fimilar parallel fringes, of fmaller dimenfions, differing in number, according to the diftance at which the fhadow was obferved, but leaving the middle of the fhadow always white. Now thefe fringes were the joint effects of the portions of light paffing on each fide of the flip of card, and inflected, or rather diffracted, into the fhadow. For, a little fcreen being placed a few inches from the card, fo as to receive either edge of the fhadow on its margin, all the fringes which had before been obferved in the fhadow on the wall immediately difappeared, although the light inflected on the other fide was allowed to retain its courfe, and although this light muft have undergone any modification that the proximity of the other edge of the flip of card might have been capable of occafioning. When the interpofing fcreen was more remote from the narrow card, it was neceffary to plunge it more deeply into the fhadow, in order to extinguifh the parallel lines; for here the light, diffracted from the edge of the object, had entered further into the fhadow, in its way towards the fringes. Nor was it for want of a fufficient intenfity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intenfity was reduced to one-tenth or one-twentieth.

Experiments on heat by Count Rumford.

Fig. 2.

Fig. 1.
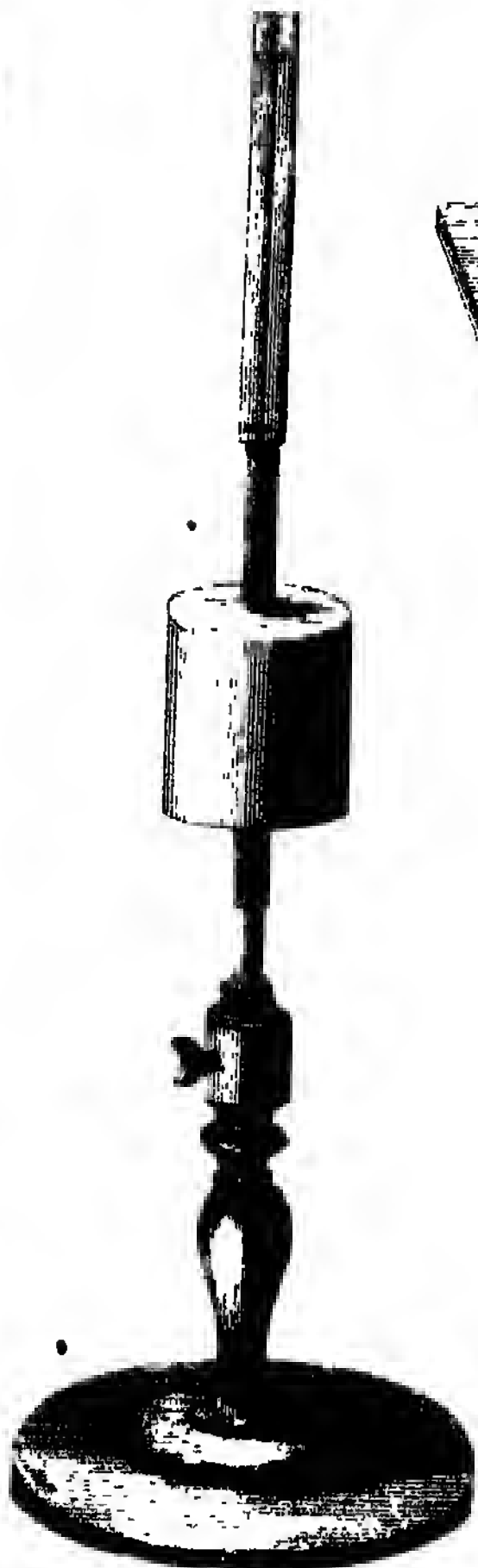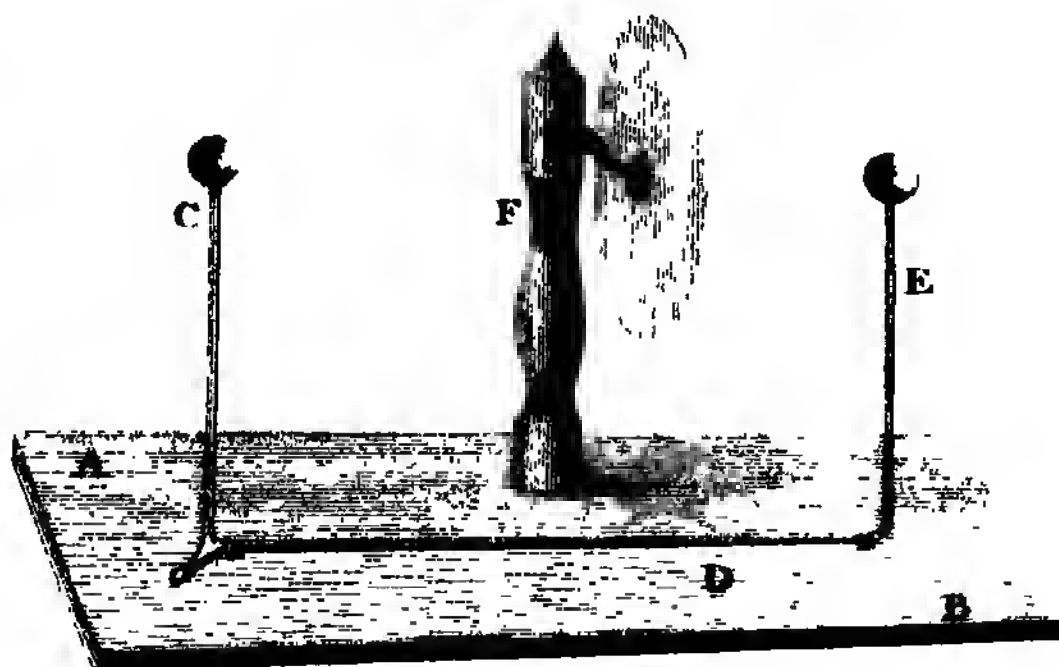
Fig. 3.

Experiments on heat by Count Rumford.
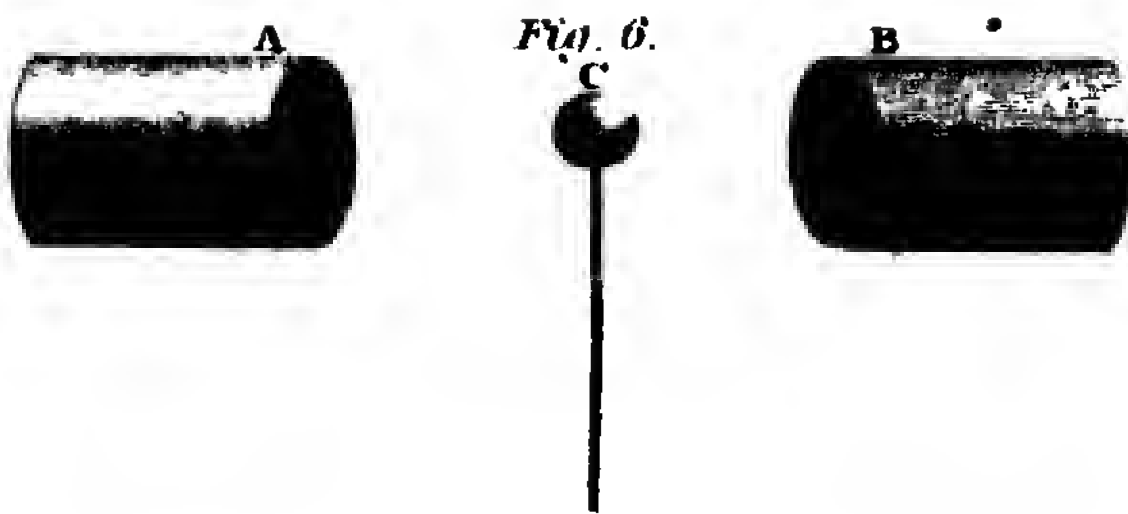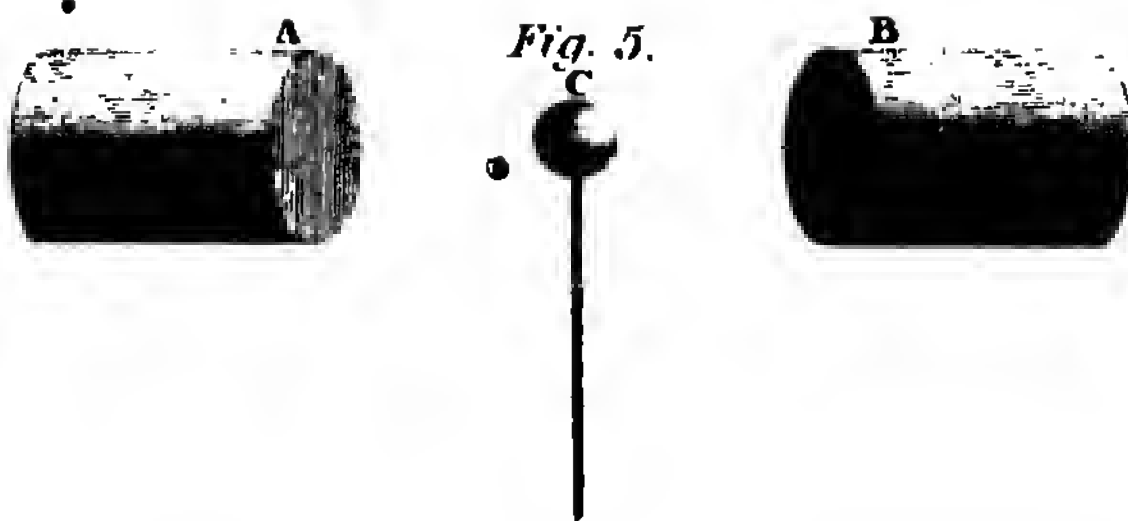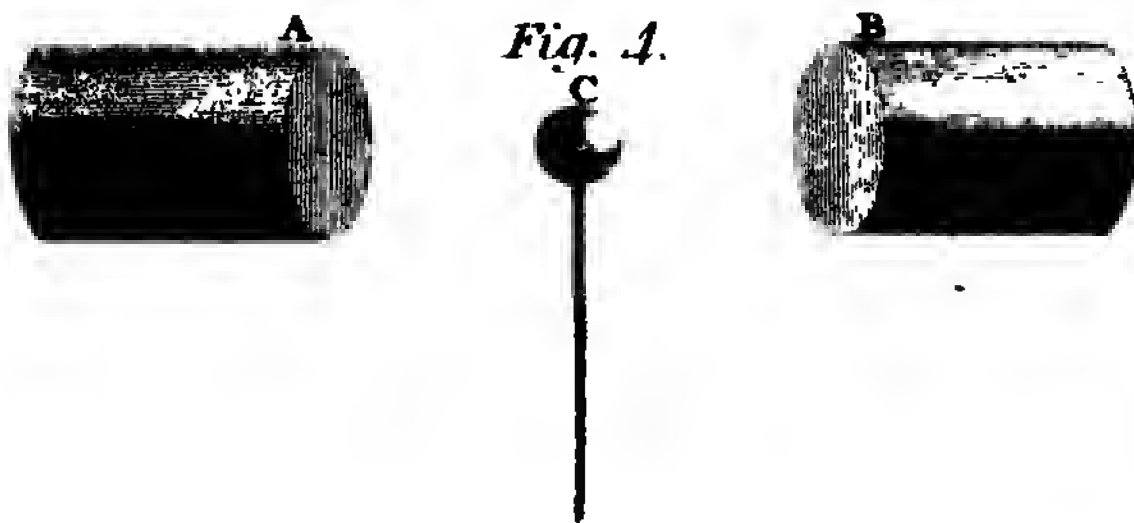
Fig. 4.

Fig. 5.

Fig. 6.

Electrical Instrument by A.W.m. Wilson.
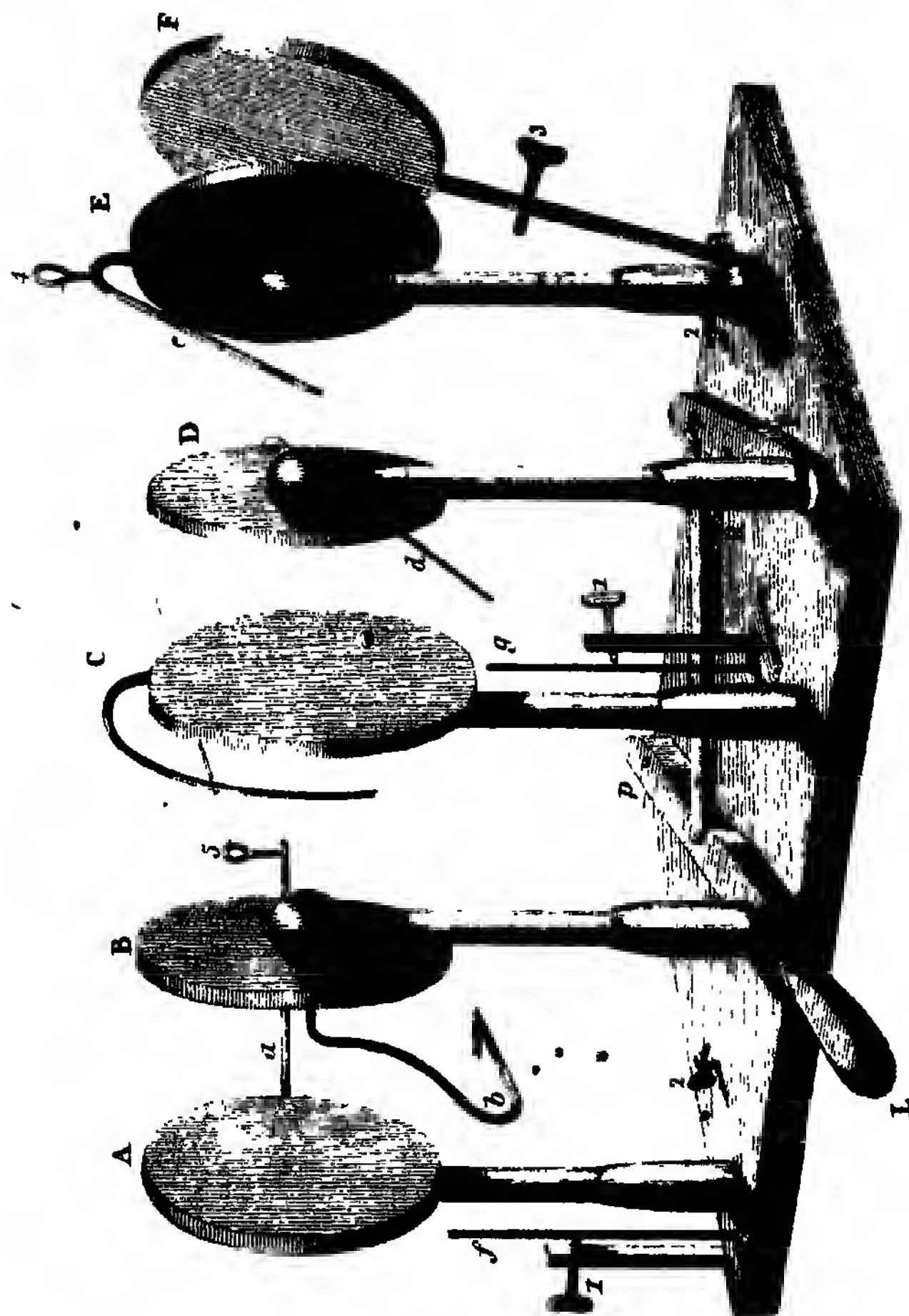
Filtering Apparatus, by Professor Parrot.
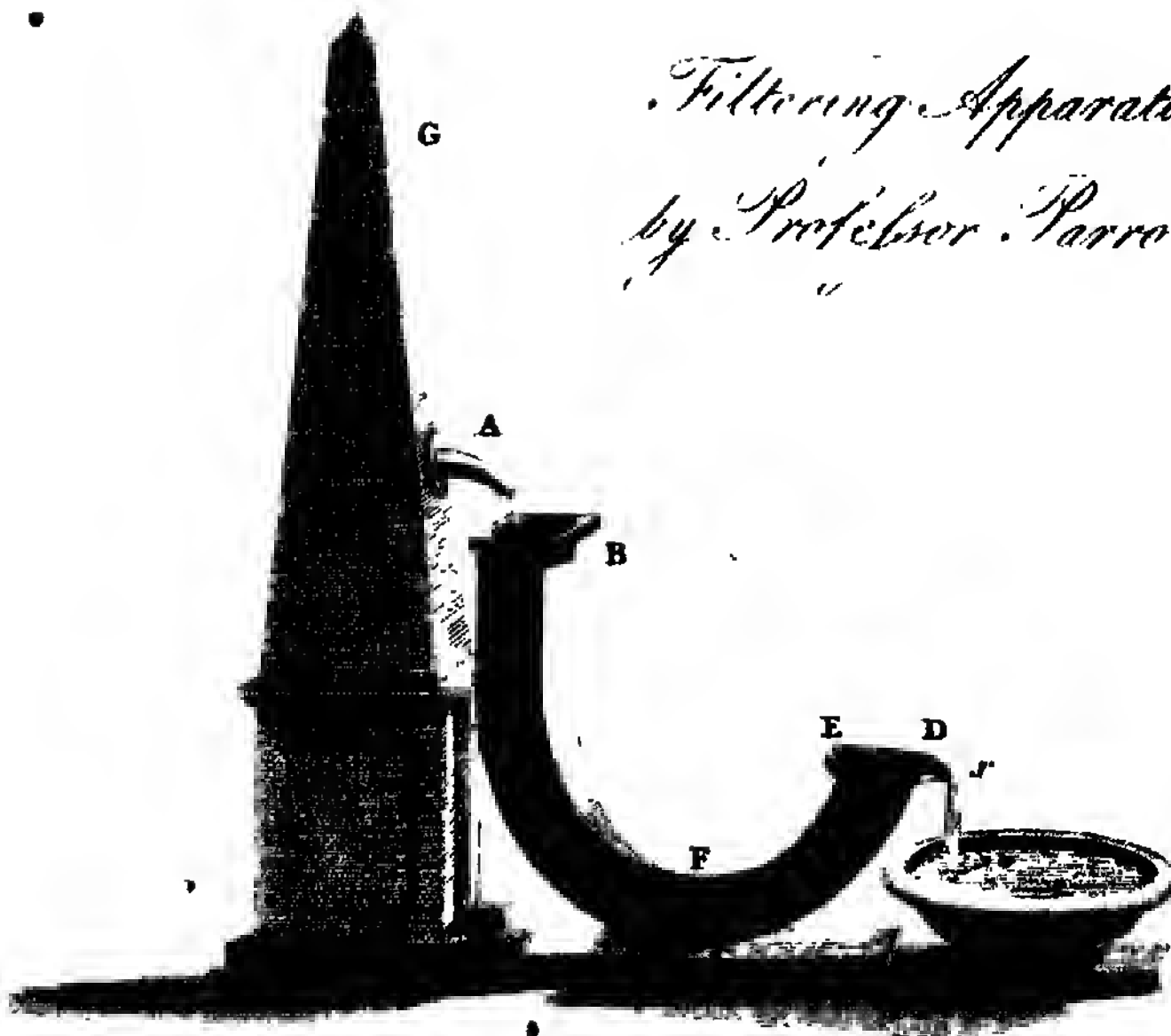
Blow-pipe by the Abbé Mclegrani.

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AND
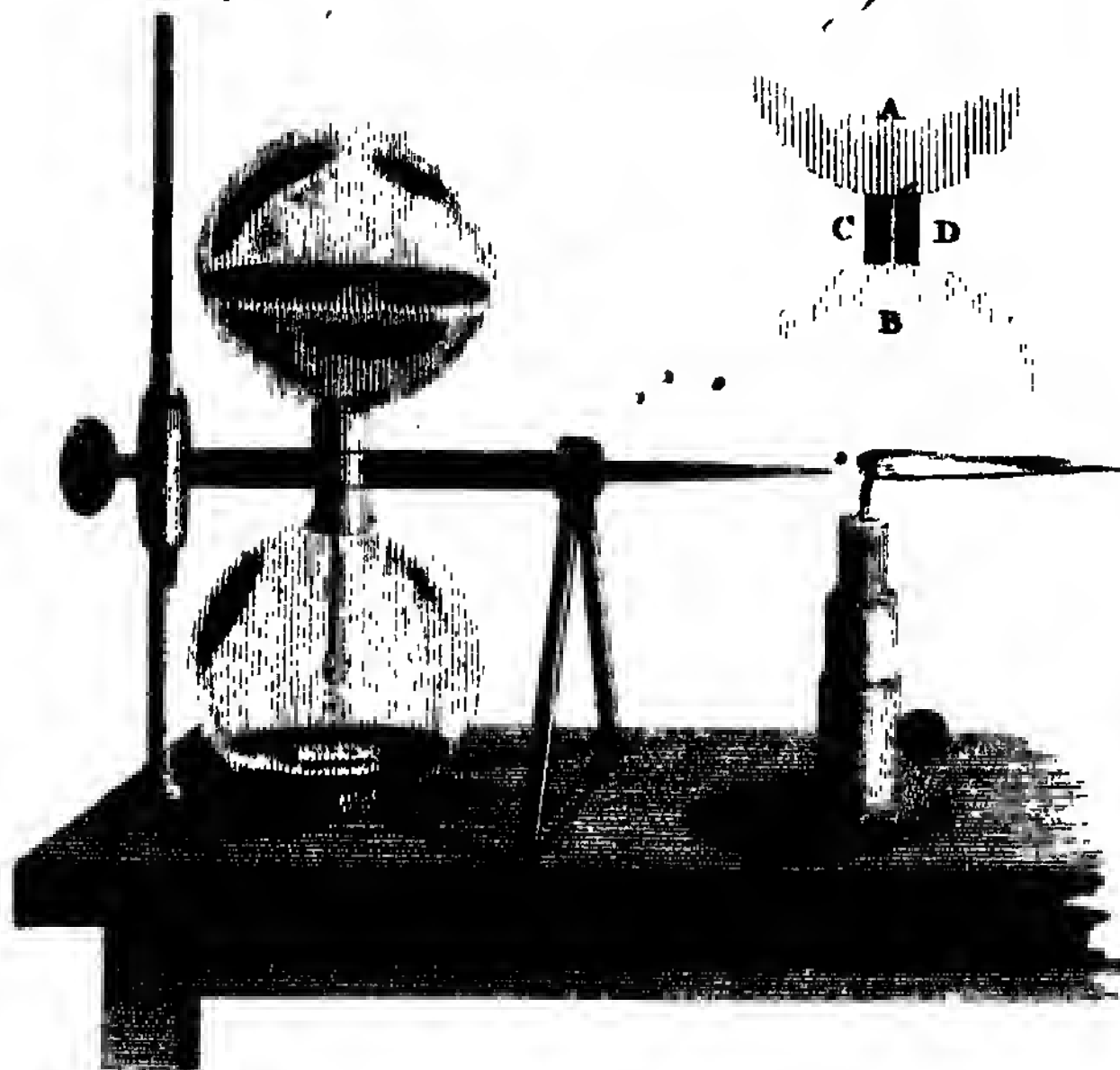
THE ARTS.

ARTICLE I.

Extract of a Letter from Count Apollos de Mouffin Poufhkin, to Charles Hatchett, Efq. F. R. S. defcribing his Method of preparing Malleable Platina. Communicated on the Requeft of the Count, by CHARLES HATCHETT, *Efq. F. R. S. and now firft publifhed.*

1. PRECIPITATE the platina from its folution by muriate of ammonia, and wafh the precipitate with a little cold water.

2. Reduce it in a convenient crucible to the well-known fpungy metallic texture, which wafh two or three times with boiling water to carry off any portion of faline matter which may have efcaped the action of the fire.

3. Boil it for about half an hour in as much water mixed with one tenth part of muriatic acid as will cover the mafs to the depth of about half an inch in a convenient glafs veffel. This will carry off any quantity of iron that might ftill exift in the metal.

4. Decant the acid water, and edulcorate or ftrongly ignite the platina.

5. To one part of this metal take two parts of mercury, and amalgamate in a glafs or porphyry mortar. This amalgamation takes place very readily. The proper method of conducting it is to take about two drams of mercury to three drams of

Purif. of platina.
1. Precip. the folution by mur. of amm.
2. Reduce to the fpongy metal, and wafh.

3. Boil in weak mur. acid, to carry off all iron.

4. Edulcorate and ignite:

5. Amalgamate with mercury:

platina, and amalgamate them together; and to this amalgam may be added alternate fmall quantities of platina and mercury till the whole of the two metals are combined. Several pounds may be thus amalgamated in a few hours, and in the large way a proper mill might fhorten the operation.

6. Mould the amalgam into pieces; which foon become folid.

6. After the amalgam is completely produced, it muft be quickly moulded in bars or plates, or any other forms that may be preferred; taking care that thefe moulded pieces fhould at leaft be half an inch in thicknefs, and of a proper length to manage them afterwards in the fire; it is alfo requifite that the moulds fhould be perfectly even and fmooth. Half an hour after the pieces are formed they begin to harden by the oxidation of the mercury, and change their brilliant metallic colour for a dull leaden one.

7. Expel the mercury, by ignition :

7. As foon as the pieces have acquired a proper degree of hardnefs to be handled without danger of breaking, which commonly takes place in a little more than an hour, place them in a proper furnace, and keep them ignited under a muffle or in a fmall reverberatory. No other precaution is neceffary in this operation but that of not breaking the pieces during their

Laftly,
Heat the platina ftrongly, and forge or laminate it.

tranfport. The mercury flies off during the heat, and the platina remains perfectly folid; fo that, after being ftrongly ignited two or three times before the bellows, it may be forged or laminated in the fame manner as gold or filver; care being taken, at the commencement of the forging or of paffing it between rollers, not to apply too great a force till the metal has

To fave the mercury on a large fcale.

acquired all its denfity. It is almoft fuperfluous to add that in evaporating the mercury from large quantities of amalgam, a proper apparatus, fuch as in the filver amalgamation, muft be employed to receive the volatilized mercury; but for fmall quantities, where the lofs of this metal is of no confequence, the furnace muft have a proper chimney to carry off the metallic vapours. When the platina comes out of the firf fire its dimenfions are about two thirteenth parts fmaller every way

Remarks on this procefs.

than the original amalgam from the mould. The whole of this operation feems to be governed by the preffure of the atmofphere and the laws of cohefive attraction: for the air is driven out from between the molecules of the platina, which by their folution in mercury are moft probably in their primitive and confequently uniform figure. It is very vifible and at the fame time a very amufing phenomenon to obferve, (during the

procefs

procefs of ignition, which is performed in four or five minutes)
how the platina contracts every way into itfelf, as if preffed by
fome external force.*

I have alfo lately obtained triple falts of muriate of platina Triple muriatic
with muriate of ponderous earth; and alfo with muriate of falts of platina,
with earths and
magnefia; and I ftrongly fufpect that every other earth except with metals;
the filiceous, and even the metals, are fufceptible of fuch triple
combinations.   I have likewife obtained a very beautiful falt —beautiful falt
of platina by the combination of foda and platina with the mu- with foda.
riatic acid; a combination which Bergman and feveral other
chemifts deny.   The beft manner of obtaining it is by diffolv-
ing the platina in nitrous acid, to which, for that purpofe, two
parts of muriate of foda and one of platina are added.   The
platina muft be made in a retort with its receiver, and after
about four fifths of the fluid have come over, the procefs muft
be interrupted, and the whole left to cool in the fand bath.
The falt cryftallizes in fine prifms, which are fometimes four or
five inches long, and either red brown, like titanium, yellow,
like amber, or of a beautiful coquelicot colour, according to
the purity of the platina.   I enclofe here my addrefs during
my abfence, and hope you will receive with indulgence the
contents of this letter.

<div align="center">

I am with great regard, Sir,
Your moft humble and obedient fervant,
COUNT APOLLOS MOUSSIN POUSHKIN.

</div>

* In the Count's letter to Mr. Hatchett, requefting him to pub-
lifh the method in the text (communicated to Mr. H. fome years
ago) the following addition is given : (in French.)
   " As foon as my amalgam of mercury is made, 1 comprefs the
fame in tubes of wood, by the preffure of an iron fcrew upon a cy-
linder of wood, adapted to the bore of the tube.   This forces out
the fuperabundant mercury from the amalgam, and renders it folid.
After two or three hours I burn upon the coals or in a crucible
lined with charcoal, the fheath in which the amalgam is contained,
and urge the fire to a white heat; after which I take out the platina
in a very folid ftate, fit to be forged."

II.

On Pepper. By THOMAS THOMSON, *M. D. Communicated by the Author.*

Vegetable chemiftry neglected.

NOTWITHSTANDING the great number of labourers who have engaged in the cultivation of chemiftry, the field of that alluring fcience is too extenfive to be fully occupied. While many fubdivifions are left entirely wafte, and others exhibit here and there only faint traces of improvement, fome fortunate fpots, either from their fuppofed importance, or from the influence of fafhion or accident, have been crowded with workmen, and cultivated with enthufiaftic eagernefs. The mineral kingdom has probably engaged the exclufive attention of nearly two thirds of the whole body of practical chemifts: The animal kingdom, in confequence of the intimate connection between chemiftry and medicine, has enjoyed a confiderable fhare of cultivation; but the vegetable kingdom, though furely not inferior in importance, and apparently rather more alluring, has till lately been greatly neglected, at leaft in Britain.

Reafons why this branch muft be always lefs accurate than mineral chemiftry.

Indeed it is not likely that vegetable chemiftry will ever arrive at the precifion which we have a right to look for in the analyfis of minerals. The conftituents of the latter feem fcarcely fufceptible of altering their ftate; but thofe of vegetables run progreffively through a regular fuite of changes.—Thus the fubftance, which in the embryo ear of corn poffeffes the properties of *mucilage*, appears in the ripened grain under the form of *fiarch.* . Between thefe two extremes there exifts an indefinite number of intermediate ftates, through all of which the vegetable matter fucceffively runs. The rapidity and completion of thefe changes depend upon a multitude of circumftances. In no cafe can they proceed exactly in the fame order and at the fame rate unlefs all the circumftances tally. Without this the conftituents of two vegetables even of the fame fpecies, cannot be in the fame ftate, and of courfe the analyfis of each, though conducted with the moft perfect accuracy, will by no means exhibit an exact coincidence.

Changes in vegetable matter after life; and by reagents.

But even fuppofing the conftituents of two vegetable bodies in every refpect the fame, ftill the analyfis of each may lead

to different refults. Vegetable fubftances not only pafs through a fuit of changes while they conftitute a part of the living plant, but many of them are ftill fufceptible of continuing the fuit even after they are feparated from the parent that produced them. Thus *gluten* when kept moift runs into *cheefe*; oil when long expofed to the fun and air hardens into *wax* or *refin,* and the milky juices of plants into *gum-refins*. Our analyfis frequently accelerates or occafions thefe changes, and even produces others altogether new. Hence the *principles* which we extract from vegetable bodies are not always the conftituents of thefe bodies; but a new fet of principles formed during the analyfis, and of courfe varying according to the nature and circumftances of the experimental inveftigation. Hence we are feldom able to form again the old vegetable compound by uniting together all the ingredients which we have extracted from it.

Thefe difficulties increafe with the complicated nature of the vegetable body; for the greater the number of conftituents is, the more liable are they to undergo alteration during an analyfis. Indeed fome vegetable principles feem incapable of exifting except in combination, and are decompofed or new modified the inftant we attempt to feparate them.

Complication of principles.

The variety of ftates in which the vegetable principles fucceffively exift, together with the difficulty of examining their properties without altering their nature, has rendered it necef-fary for chemifts to apply the names of them with greater latitude than is ufual in other departments of the fcience. A general refemblance, indeed, in the moft ftriking properties, feems to have been thought fufficient to entitle vegetable principles to the fame name. If we examine all the bodies termed *gum,* we fhall find them running on the one hand into *ftarch,* and on the other into *fugar;* and forming a pretty long feries, having fugar and ftarch at the two extremities: no two of the fubftances conftituting the feries are exactly the fame; but the fame name is ufually applied to all thofe that have a general refemblance. This will be eafily feen by the following table, in which I have fubdivided the feries into five genera.

Loofe nomenclature of vegetables.

Genus I. Sugar - -
{ 1. COMMON SUGAR
{ 2. Sugar of grapes
{ 3. Sugar of beet.

Inftances in fugar, gum, ftarch.

II. Sarcocoll

II. Sarcocoll -
- 1 Manna
- 2. Liquorice
- 3 Common farcocoll
- 4 Saccharine of malt.

III. Gum - -
- 1. Mucilage of roots
- 2. Cherry-tree gum
- 3. GUM-ARABIC
- 4. Gum of barks.

IV. Gluten -
- 1. Gluten of barley
- 2. Gluten of wheat
- 3. Cafeous principle.

V. Starch -
- 1. Gum tragacanth
- 2. Gum of lichens.
- 3. Common ftarch.

Thefe terms are generic. The terms *gum, ftarch, fugar,* &c. in vegetable chemiftry are not to be underftood as the names of peculiar fubftances, nor even as *fpecies;* but merely as *genera,* and nearly fimilar to the terms metals, acids, alkalies, &c. in the other departments of chemiftry.

Subject introduced: Pepper. The preceding obfervations will ferve, I truft, as an apology for the imperfection of the following remarks on *pepper.* I offer them to the public not as an analyfis of that vegetable fubftance, but as an account of fome of the properties of its moft remarkable conftituents.

Defcription. I. BLACK PEPPER is the fruit of an Eaft Indian plant, the *piper nigrum,* and is too well known to require any particular defcription. The outer coat of the pepper-corn is brown, and a good deal fhrivelled. Its tafte is not nearly fo pungent as the inner part; of, courfe it contains lefs of the peculiar principle to which pepper owes its tafte and fmell.

Maceration in cold water. II. When pepper is macerated in cold water it does not lofe its fhrivelled appearance; a proof that the corns are impregnated with an oily fubftance, which prevents them from abforbing water. The liquid very foon acquires a fine deep reddifh brown colour, but retains its tranfparency. *White pepper,* which is known to want the outer coat, communicates no fuch colour to water; the colouring matter then muft refide in . the outer coat.

The watery infufion thus obtained poffeffes the fmell and tafte which are peculiar to pepper. Like moft other *extracts,*

it .

it has the property of giving a red colour to vegetable blues. A very great quantity of water fucceffively applied is nee effary to exhauft the pepper of its colouring matter: but the fmell and tafte of pepper become lefs and lefs ftrong in thefe infufions, and at laft altogether imperceptible, leaving the infufions infipid, or flightly fweetifh.

1. Thefe cold infufions contain a peculiar *extractive* matter, which feems to refide in the outer coat of the pepper-corn. In the firft infufions their matter is united to the fubftance, in which the tafte and fmell of pepper refide, and occafions its folubility in water. In the laft infufions, if we judge from their appearance, it feems to be mixed with a mucelaginous fubftance.

2. If we mix the infufion of nut-galls with the cold infufion of pepper, no fenfible change is produced; but in a decoction of pepper, it produces a copious flaky precipitate. Hence we learn that there is a fubftance in pepper infoluble in cold water, but feparated by means of boiling water. This fub-ftance, as fhall be afterwards fhown, is a fpecies of *ftarch*.

3. When pepper is macerated in alcohol it communicates a light yellowifh green colour to the liquid, which becomes at the fame time fully impregnated with the peculiar hot princi-ple which characterizes pepper. By diftilling this tincture in a retort, the alcohol is obtained colourlefs, but of a decidedly peppery flavour. Towards the end of the diftillation, the liquid in the retort becomes muddy, and depofites a greenifh matter, part of which may be obferved alfo trickling down the fides of the receiver like drops of oil. The refidual liquid is yellow, but nearly infipid. This green matter is the fubftance to which pepper is indebted for its tafte and fmell. Its pro-perties are analogous to thofe of the *volatile oils*.

4. Thefe three bodies, namely, *extractive*, *ftarch*, and *oil*, I confi.ler as the moft important ingredients of the pepper-corn. Let us examine the properties of each, beginning with the oil, which is obvioufly the effential ingredient.

III. The colour of the oil of pepper is grafs green. When firft obtained it is of the confiftence of turpentine, but it gra-dually hardens by expofure to the air. When moderately heated, it gives out a liquid oil of a yellowifh green colour, and leaves a folid mafs, fimilar nearly to a refin. When thrown into water, it finks to the bottom of that liquid.

Its

Its taſte is intolerably hot, and preciſely ſimilar to that of pepper. So is its ſmell.

When heated to 100°, it ſoftens; it melts at 148°, evaporates a little above 212° in a white ſmoke, which ſmells like tobacco ſmoke, irritating the throat and exciting coughing. Evaporated to drynefs on a glaſs plate, it leaves a yellow trace behind it. When ſuddenly heated, it boils violently, and the vapour burns with a clear white flame without any ſmoke.

It gives a greaſy ſtain of a green colour to paper. At 500° the greaſy appearance is removed, but the green mark ſtill continues, unleſs the heat be ſufficient to char the paper. Hence I think it follows, that the colouring matter of this oil is a ſubſtance entirely diſtinct from the oil itſelf.

The oil of pepper is inſoluble in water; alcohol and ether diffolve it readily; the ſolution is light green: alcohol holding it in ſolution, acquires a very fragrant odour, preciſely ſimilar to that of oil of lavender. When water is added to this ſolution, the whole becomes milky, and paſſes in that ſtate through the filter. On ſtanding ſome weeks, light green flakes ſubſide, but the milky opacity is permanent.

Alkalies have no ſenſible action on this oil while cold. When thrown into liquid potaſh it ſwims on the ſurface. If the liquid be heated, the oil becomes brown and acquires greater confiſtence. At the ſame time it evaporates partially, diffuſing around the peculiar odour of pepper.

Nitric acid diffolves it with efferveſcence, the ſolution is yellowiſh brown, and a waxy matter ſwims on the ſurface. This acid acts in the ſame manner on other volatile oils.

The oxygenized muriatic acid deſtroys the green colour and makes it yellowiſh white. But it ſtill retains its former taſte.

Theſe properties are ſufficient to authorize us to refer this green matter to the genus of volatile oils It is not however a pure oil, for beſides the colouring matter already mentioned, it obviouſly contains a ſubſtance which approaches to the genus of refins in its properties.

IV. The EXTRACTIVE of pepper is procured by macerating the pepper-corns in cold water. Like the other ſpecies of this difficult genus of vegetable principles, it is ſcarce poſſible to obtain it in a ſtate of abſolute purity. But if we macerate the pepper-corns whole in ſucceſſive portions of water, till the liquid loſes the peppery flavour, and then pour on them freſh

4   water,

water, we obtain an infusion which I believe holds in solution scarcely any thing but extractive. If this infusion be evaporated slowly in a steam heat, it leaves a brown refiduum, which I confider as extractive of ftarch not far from purity.

1. The infusion is insipid or slightly sweetish, and of a fine reddish brown colour. As the evaporation advances, the colour deepens, and the liquid acquires an acrid flavour, similar to that of scorched vegetable matter. This flavour, which indicates a commencement of decompofition, is evolved at a very moderate temperature.

2. If the evaporation be condnfted rapidly the extractive is opaque, dark brown, and has a perceptible tafte, and delequefces or at leaft attracts moifture when firft expofed to the atmofphere. But by very flow evaporation I have obtained it in fine femi-tranfparent brown fcales, which are infipid, brittle, and not altered by expofure to the air.

3. This extractive diffolves readily in water, but not in alcohol. If it be repeatedly diffolved in water, and the folutions evaporated to drynefs, a fmall portion of it becomes infoluble in that liquid; but the greater part continues foluble after the procefs has been repeated occafionally even for months.

4. This extractive is precipitated from water by moft of the metallic falts and by feveral of the earthy folutions.

It is thrown down in brown flakes by lime-water ftrontian water and alum. Barytes water deepens the colour, but occafions no fenfible precipitate; nor is any precipitate produced by filicated potafs or the magnefian falts.

It is precipitated brown red by the nitro-muriate of gold, the nitrate of filver, mercury, lead and bifmuth, and by the muriate of tin and antimony. When mixed with the infufion of litmus, the colour becomes red. Ammonia reftores the original colour, and at the fame time throws down a copious blue lake.

5. When a current of oxymuriatic acid is paffed through the infufion of pepper, the brown colour is fpeedily converted into a pale yellow, and the extractive precipitates in white flakes. The eafieft method of making this experiment is the following: Put a quantity of the hyperoxymuriate of potafh into a fmall retort, having a long neck (or a flafk provided with a bent glafs tube ground into it) and pour upon it a portion

tion

tion of muriatic acid. Let the infusion of pepper be put into a tall glafs veffel, and plunge the beak of the retort to the bottom of it. A current of oxymuriatic acid is difengaged from the falt, and paffes in bubbles through the infufion for a confiderable time.

When ammonia is added to the infufion thus made white, the original red colour is reflored. The white flakes precipitated by oxymuriatic acid gas are infoluble in cold water.

Decoction of pepper affords with tan, a precip. foluble in hot, but not in cold water.

V. That pepper contains a fpecies of flarch, I conclude from the following experiment, which I have frequently repeated. When the decoction of pepper is mixed with the infufion of nut-galls, a copious precipitate falls in reddifh brown flakes. If this liquid be heated to the temperature of about 120°, the precipitate is re-diffolved, but appears again when the folution cools. Now the only fubflance which poffeffes the property of forming with tan, a precipitate nearly infoluble in cold water, but very foluble in hot water, is flarch. Tan indeed throws down gluten, but the precipitate is not re-diffolved in the application of heat. It throws down caoutchouc and fome of the gum-refins, but the precipitate is fcanty and probably owing to the extraneous matter. The precipitate which it forms with gelatine and albumen cannot even by the moft carelefs obferver, be confounded with the compound of flarch and tan. But befides this property, which I confider as characteriftic, the flarch of pepper, agrees with common flarch in the phenomena which it exhibits with the different chemical reagents.

This property is characteriftic of flarch.

As thefe phenomena have not yet been detailed by chemical writers, it may be neceffary to give a fhort fketch of them in this place.

Phenomena of flarch with reagents.

1. Starch is one of thofe vegetable bodies that are leaft liable to decompofition. It conftitutes one of the moft important articles of food, and acts an important part in the production of fermented liquors. The obvious properties of common flarch are too well known to require any defcription.

Starch not eafily decompofed.

2. Neither alcohol nor ether nor water are capable of diffolving it. The laft liquid when affifted by a boiling heat, readily unites with it, and forms a kind of jelly, which may be diffufed through boiling water; but when the mixture is allowed to ftand a fufficient time, the flarch flowly precipitates to the bottom. By drying the compound of flarch and water, a brittle

Not foluble in alcohol, ether, or water.

Forms a jelly with hot water which is little changed.

a brittle fubftance is obtained, differing in appearanre from common ftarch, but exhibiting nearly the fame properties with re-agents. The apparent difference is probably owing to a portion of water remaining united to the ftarch.

3. When ftarch is triturated with the hot infufion of nut-galls, a complete folution is effe&ted. The folution is tranf-parent and rather lighter coloured than the infufion of galls. When cold it becomes opaque, and a copious curdy precipi-tate falls. —Completely foluble in hot infuf. of galls, and precip. by cold.

The infufion of nut-galls, which I am accuftomed to em-ploy in all my experiments, except when the contrary is ex-prefsly mentioned, is made by boiling together one part of galls in coarfe powder and two parts of water in a glafs retort. When cold, the liquid part is decanted into a glafs phial. It is at firft muddy, and opake; but on ftanding, a fediment falls, and a tranfparent liquid remains of a deep brown colour, which conftitutes my infufion. An ounce meafure of this in-fufion, when evaporated to drynefs in a glafs veffel placed on a tin-plate box, heated by fteam, leaves a brown refidue, which weighs 68 grains. This refidue confifts chiefly of tan; for the greateft part of the extra&tive gradually feparates from the infufion in the ftate of a brown, tough, imperfe&lly folu-ble membrane. Neither *extra&ive* nor pure *gallic acid* has any effe&t upon the deco&tion of ftarch. Hence the precipitate is obvioufly produced by the fole a&tion of tan upon the ftarch. The infufion of galls as prepared was *tan*.

4. Twenty-four grains of ftarch were triturated with half an ounce meafure of the infufion of galls, and mixed with about five ounces of hot water. A complete folution took place; but on cooling, the liquid became opaque, and a precipitate fell, which dried by a fteam heat, weighed 35 grains. The refidual liquor had a light yellow colour and an aftringent tafte. When evaporated to drynefs, it left a refiduum that weighed 17 grains. This refiduum contained ftarch, for it was not completely foluble in alcohol. In this experiment fome lofs muft have been fuftained during the trituration. For the folid matter obtained weighed only 52 grains; or fix grains lefs than the 24 grains of ftarch and the 34 grains of folid matter in the infufion of galls. The following is more to be depended on : After various trials, I found that ftarch and tan are capable of uniting in different proportions. But the precipitate is leaft foluble when ¼ oz. meafure of infufion of galls is ufed for every 24 grains of ftarch.—I took 24 grains of ftarch, boiled them Experiment of the union of ftarch and tan.

them in a flafk with five ounces of water, and then added ½ oz. meafure of the infufion of galls. On cooling, a copious precipitate fell, and the liquid remained only faintly coloured. The precipitate dried in a fteam heat weighed 31 grains, and the refidual liquid left 11 grains of refidue. The whole amounted to 42 grains; which is very nearly equal to the 24 grains of ftarch and 17 of tan employed; the compound in this cafe confifts of

$$58.5 \text{ ftarch, or nearly } 3 \text{ ftarch}$$
$$41.5 \text{ tan } - - - - 2 \text{ tan}$$
$$100.0 \qquad\qquad 5$$

The compound of ftarch and tan is of a light brownifh yellow colour, femi-tranfparent and brittle, and has a good deal of refemblance to common farcocoll. Its tafte is aftringent ; it feels glutinous between the teeth, like gum. It is very imperfectly foluble in cold water, but hot water diffolves it abundantly. Alcohol digefted on it acquires a brown colour; but is incapable of feparating the whole of the tan from the ftarch. When heated, it froths, fwells and melts, and then burns with a clear flame, leaving like ftarch a fmall portion of white afhes behind it.

Infufion of ftarch treated with earths.— 5. To afcertain the effect of the earths and metallic oxides on ftarch, an infufion of it was formed by triturating 24 grains of ftarch with $4\frac{1}{2}$ ounces of water, and then boiling the mixture for fome time. The decoction thus formed, is nearly tranfparent, and of a flight opal colour. When fet afide, at leaft a month elapfes before the ftarch begins to fubfide.

When lime water is mixed with this decoction, no change is produced; neither is any perceptible alteration occafioned by ftrontian water; but barytes water throws down a copious white flaky precipitate. This precipitate is re-diffolved by muriatic acid, but appears again on ftanding unlefs a confiderable excefs of acid has been added. Yet muriate of barytes occafions no change in the decoction of ftarch.

—with metallic falts : no effect. No metallic falt feems to have the effect of throwing down ftarch from its decoction. The following were tried :

Nitro-muriate of gold, platinum;
Nitrate of filver, mercury, lead;
Muriate of tin, acetite of lead;
Salts of copper, iron, zinc;
Ammoniated nickel and cobalt.

6. When

6. When potafs is triturated with ftarch, and a fmall quantity of water added, the whole affumes on ftanding the appearance of a femi-tranfparent jelly. On adding water, an opal coloured folution is obtained, from which the ftarch is readily thrown down by an acid. When muriatic acid is employed, a peculiar aromatic odour is exhaled.

When the infufion of galls is dropt into the folution of ftarch in potafs, a yellowifh white precipitate appears, but is immediately re-diffolved, and the liquid remains opake and of a dark brown colour. On adding muriatic acid, a copious precipitate falls, refembling the compound of ftarch and tan.—Nitric acid occafions no precipitate, neither does ammonia.

The decoction of ftarch is neither altered by potafh, carbonate of potafh, nor ammonia.

7. Sulphuric acid diffolves ftarch, and abundance of charcoal is precipitated. Diluted fulphuric acid, when affifted by heat, diffolves it without decompofition. Sulphureous acid has no effect upon it.

Diluted nitric acid firft reduces ftarch to powder, and then diffolves it, with the exception, of fome waxy matter, which fwims on the furface. During the folution, fome nitrous gas is exhaled.

Strong muriatic acid diffolves ftarch flowly, and without effervefcence. When the ftarch does not exceed $\frac{1}{16}$ of the acid, the folution is colourlefs and tranfparent, but if we continue to add ftarch, a brown colour foon appears, and the acid lofes a portion of its liquidity. Its peculiar fmell is deftroyed, and replaced by the odour which diftinguifhes corn mills.

Acetic acid does not diffolve ftarch.

8. Alcohol feparates ftarch in part from its decoction. Potafh diffolved in alcohol occafions a copious white precipitate, which is re-diffolved on adding a fufficient quantity of water. Alcohol digefted on fulphuret of potafs, occafions a flaky precipitate in the decoction of ftarch: this precipitate has fometimes an orange colour.

Such are the properties of common ftarch: the ftarch of pepper poffeffes them, excepting only that the colour of its precipitates is peculiarly modified by the other ingredients of pepper, from which it is not poffible to free the ftarch completely.

*A very confiderable portion of the pepper-corn feems to confift of ftarch, chiefly united to the oil of pepper. When a

pepper-

Margin notes:

Treatment of ftarch with potafh. Solution. Separation by acids. The infufion of ftarch not affected by alkalies. Starch diffolved by the ancient mineral acids. Phenomena.

Experiments with alcohol.

Starch of pepper has the preceding properties. It is united to an oil in the pepper-corn.

pepper-corn is cut in two, we find it compofed of two coats. The outermoft brown fhrivelled coat is eafily feparated by fleeping the pepper in water. The fecond coat is much thinner and lighter coloured; it does not feparate by maceration. Beneath this coat is a thick zone of pale green matter, apparently ftarch united to oil. This is fucceeded by a thin yellow zone, feemingly of nearly the fame compofition. Within this is a fmall fpherical fpace, fometimes hollow, but moft commonly filled with a foft white fubftance like the pith of trees. All the zones have the peppery tafte. The outer coat has the flavour, but little of the heat of pepper.

III.

Letter from the Abbé BUÉE *on Mr.* ROMÉ DE LISLE'S *and the Abbé* HAUY'S *Theories of Cryftallography.*

(Concluded from p. 39.)

SYNTHESIS is grounded, as I mentioned, on the fact, that all well formed cryftals are terminated by plain furfaces.

Primitive forms: Since there exift primitive forms, there muft alfo be fecondary forms, for the one fuppofes the exiftence of the other.

Secondary.   The fecondary forms are fuch, that fections can be made only parallel to the fides of the primitive; and when the primitive has been produced by thefe fections, the divifion being continued the integrant particles are obtained.

Laws of the ftructure of cryftals, and theory thence refulting.   The mineralogical analyfis defcends from the fecondary to the primitive form, and from the latter to the integrant particle; juft fo the mineralogical fynthefis afcends from the integrant particle to the primitive, and from thence to the fecondary forms. A cryftalline edifice is therefore raifed by means of the integrant particles. What are the laws of this extraordinary architecture? By laws I mean the difpofition of the laminæ, not the means employed by nature to execute the curious ftructure.

Laws muft exift, 1ft, for the formation of the primitive, and 2dly, for the conftruction of the fecondary form. The primitives are either fimilar to their integrant particles, or they are not. If they are not, their forms muft be parallelopipedons, [*]

pedons, and their laws of formation very fimple; for there will be the fame number of integrant particles in each row, as there are rows in each lamina, as there are laminæ in the primitive form. It is eafy to conceive that all the joints perfectly coincide with each other, and form continued planes; neither will there be any vacuity left between the particles. If the primitive be not fimilar to the integrant particle, then the fimplicity of the former cafe difappears. I have already ftated that there are three forms of integrant particles; the tetraëdron, the triangular prifm, and the parallelopipedon. There are alfo fix primitive forms; the parallelopipedon, the octaëdron, the tetraëdron, the regular hexaëdral prifm, the dodecaëdron bounded by rhombs all equal and fimilar, and the dodecaëdron with triangular fides and formed by two right pyramids united bafe to bafe. Of thefe fix primitive forms, there are only the parallelopipedon and the regular hexaëdral prifm that can exactly fill up a fpace without leaving any vacuity. The integrant particles of the former are parallelopipedons; of the latter, triangular prifms. As to the other four primitive forms, their integrant particles are tetraëdrons. The dodecaëdron bounded by rhombs is produced by twenty-four fimilar tetraëdrons without any vacuity between them; the octaëdron and tetraëdron are formed by tetraëdrons leaving octaëdral vacuities; and the dodecaëdron bounded by triangles, to be formed of tetraëdrons, muft imply fections parallel to more than fix planes; which perfectly coincides with obfervation.

Thefe vacuities, whofe exiftence muft be admitted in the integrant particles, as well as between thofe particles when forming a primitive, give rife to the following reflections:

When the elements of a fubftance are chemically combined, that fubftance is homogeneous. Let us fuppofe a cryftal of fuch a fubftance to be fubdivided into fmall parallelopipedons equal and fimilar; as the fubftance is homogeneous, and thefe little parallelopipedons having no vacuities between them, it is evident the elements that compofe them are equal in number and proportion. We will next fuppofe the cryftals of this fubftance can be divided by fections parallel to fix planes. In that fuppofition, nineteen or twenty different fpecies of parallelopipedons can be produced. Among thefe fpecies fome will be fimilar, others not; but none of the fpecies will be exactly

actly parallel to each other. We will proceed on two fimilar cryftals of the fame fubftance and equal in folidity, dividing the firft into one fpecies, the other into a different fpecies, of parallelopipedons, equal in folidity but not in furface; and let the divifion of each be pufhed to its laft term. But as we are come by fmooth fections to parallelopipedons of different fpecies, thofe fections have alfo produced their differences: but by fuppofition thefe parallelopipedons are the refult of the laft poffible term of divifion without deftroying the chemical compofition, and being equal in folidity, though not in furface, they cannot contain each other; therefore if their differences are not integrant parts of both, thefe differences muft ceafe to be homogeneous, and we come to a fort of chemical decompofition. It is true we cannot execute this exceffive divifion, but we can form a very correct idea of it. If the little parallelopipedons contain two forts of elements, their differences will alfo, but alfo in different proportions; and, fir, if you will turn to Berthollet's *Refearches on the Laws of Affinities*, you will fee him in all his experiments proving, that however perfectly a chemical decompofition may have been made, the refults will always contain a certain portion of thofe fubftances from which it was the object of the operation to feparate them. If thefe reflections, Sir, are well grounded, do they not give us hopes, and perhaps fhow the poffibility, of defcending from the integrant particles to the conftituent particles? This fecond refearch is of the fame nature as the firft. It is more than probable that the conftituent particles themfelves are divifible, having no determined figure, but are aggregations, fubject to the fame laws as the integrant particles. The object of the natural philofopher is not to difcover the forms of the ultimate particles, but to determine their refpective pofitions; which, if ever they could be determined in the integrant particles and their component parts, the grand problem of chemical affinities would be fully folved; and fhould fuch ever be the cafe, to the Abbé Haüy's theory would be due the merit. The *Encyclopædia Britannica*, under the article *Chemiftry*, in the Supplement, p. 396, fays:

" This theory, to fay no more of it, is, in point of ingenuity, inferior to few; and the mathematical fkill and induftry of its author are entitled to the greateft applaufe.

" But

" But what we confider as the moft important part of that philofopher's labours, is the method which they point out of difcovering the figure of the integrant particles of cryftals; becaufe it may pave the way for calculating the affinities of bodies, which is certainly by far the moft important part of chemiftry. This part of the fubject, therefore, deferves to be inveftigated with the greateft care."

But I return to the point whence this digreffion carried me, to the vacuities left between the integrant particles in the conftruction of a primitive form. The Abbé confiders them as filled either by the water of cryftallization or by fome other fubfiance. It is not an admiffible fuppofition that this other fubfiance is compofed of the fame elements as the integrant particles, but in different proportions? At leaft, fuch is tha conclufion I fhould be tempted to draw after reading Bertholñ let's excellent Refearches on Affinities.

I fhall now proceed to the laws of formation in fecondary cryftals. It is eafy to deduce them from thefe two facts: *viz*. 1ft, That the fides of the fecondary cryftals are planes; 2dly, That they divide by fmooth fections parallel to the fides of their primitive form.

Let us take a rhomboid of carbonate of lime for example. If on one of the fides of the rhomboid I wifhed to raife a pyramid, I fhould lay laminæ of rhomboidal particles upon each other. Thefe laminæ would decreafe in furface until the laft is reduced to a fingle rhomboid. Thus the fecond lamina contains fewer particles than the firft, the third fewer than the fecond, and fo on. As the faces of thefe pyramids are always to be planes, the fucceffive decrements of the laminæ muft be equal; that is to fay, the fecond lamina is lefs by one range in every direction than the firft, and the third than the fecond, &c. If the decrement is more rapid; that is to fay, if two or three ranges are fubtracted in the fecond lamina, the fame number will be fubtracted from the third, and fo on fucceffively till the pyramid is completed. As the fections are to be fmooth, the joints muft form one continued plane; therefore the ranges and even the particles at the joints muft not encroach on each other: hence it follows *that the number of ranges fucceffively fubtracted from each lamina can never be incommenfurable*; that is to fay, the decrement may be 1, 2, 3, 4, &c.; but never $\sqrt{2}$, $\sqrt{3}$, &c.

Thefe are the decrements parallel to the edges, or, as the Abbé calls them, *decrements on the edges.* But they may take place in a parallel with the diagonal of the faces of the primitive; they are then called *decrements on the angles,* becaufe the diagonals are drawn from one angle to the oppofite angle. This fecond fpecies of decrement follows the fame laws as the firft.

· There is a third fpecies, called by our author *intermediate decrements.* In this cafe they are neither parallel to the edges nor to the diagonals of the faces, but to intermediate lines, which if prolonged would interfect both the edges and diagonals, but otherwife they follow the fame laws as the two firft. It is a general law, therefore, *that in all cafes the laminæ decreafe in arithmetical progreſſion, and its ratio or the number of ranges fubtracted is always commenfurable.*

The particles of which the laminæ are compofed are to be confidered as parallelopipedons; not that the integrant particles always have this figure; but if they have it not, they muft leave vacuities between them, and each vacuity being added to its correfponding particle, will complete the parallelopipedon. If this was not the cafe, the faces of the fecondary cryftals would not be planes, nor could they be fplit fmoothly in any direction. Thefe little parallelopipedons which compofe the fubtracted ranges are what I called above, after our author, *fubtractive particles.*

I fuppofed the conftruction of the fecondary form only to take place on one of the faces of the rhomboid; but what was faid relative to that face is applicable to all the others. It is alfo to be remarked that different laws of decrement may affect the different faces; even further, different laws may fuccefſively affect the fame face. Hence a diverfity of forms arife fcarcely credible to a perfon unacquainted with the doctrine of combinations. The Abbé Haüy has calculated, "that confining one's-felf to decrements by 1, 2, 3, or 4 ranges, and not taking intermediate or mixt decrements into account, the rhomboid is capable of 8,324,604 varieties of cryftalline forms.

It is an important remark, that whatever may be the variety of form, the forms (in complete cryftals) will always be fymmetrical. There are two forts of fymmetry, the perfect and imperfect. In the perfect, the right is fymmetrical with the left, and the top with the bottom; but in the imperfect, the

top

top is not fymmetrical with the bottom. This latter fpecies of Laws of the ftructure of cryftals, and theory thence refulting. fymmetry appears, by general obfervation, to be excluftvely appropriated to cryftals that become electrical by heat; that is to fay, which being expofed to the heat of the fire, or plunged into hot water, acquire the electric power. Thefe cryftals, the tourmaline, for example, acquire a pofitive electricity on one fide, while on the fide diametrically oppofite their electricity becomes negative; and all obfervations hitherto made give us reafon to conclude that thefe fides are never fymmetrical, and are always produced by different or fewer laws of decrement. " Hence," fays the Abbé, " by mere infpection it is eafy to point out which is the fide that will give the pofitive and which the negative electricity." (Vol. i. p. 237.)

The aftonifhing variety in the cryftalline forms leads us naturally to afk, What can be the caufe of this variety? This queftion has not been treated by the Abbé: allow me, Sir, to fubmit a few ideas on the fubject for the opinion of mathematicians.

Firft caufes, I repeat, are not the object of this difcuffion. I ftate the queftion thus: Why does the fame fubject cryftallize in fuch a variety of forms, always fymmetrical and always terminated by planes?

The folution of this queftion feems to require three conditions: 1ft, That the particles of the fubftance diffolved in the fluid all leave the ftate of reft at the fame inftant, to form the cryftal by their aggregation: 2dly, That, while thefe particles are in the act of drawing near to each other, no foreign power fhall imprint on them any other motion than a common motion, whether it be in a ftraight line, or rotary round their common centre of gravity: 3dly, That the particles all arrive at the ftate of reft at the fame inftant, which takes place when the act of cryftallization is finifhed. The fecond condition is neceffary, and infers the firft and third. The natural confequence of thefe conditions will be, that the aggregation of the particles will only take place conformably to a law acting equally on all of them, whatever may be the law.

Since they all leave the ftate of reft at the fame inftant, they are in equilibrio previous to that inftant. Since they all arrive at the ftate of reft at the fame inftant, they are in equilibrio after that inftant: but when particles that are acted upon by

no other force than that which they exercife on each other, are in equilibrio, they are in the cloſeſt poſſible union that concomitant circumſtances will permit. If the particles were in equilibrio previous to their leaving the ſtate of reſt, ſome-thing muſt have obſtructed their approach. Let us ſuppoſe that *ſomething* to be the interpoſition of another ſubſtance, and that ſo long as the interpoſition remains equilibrium is main-tained. But this can only be the caſe, in as much as the whole of the particles of the interpoſed ſubſtance are in equilibrio with the whole of the particles diſſolved and about to leave the ſtate of reſt, which in the future I ſhall call the *proper particles*. If by any cauſe which acts uniformly on the whole ſurface of the diſſolving fluid any of the interpoſed particles are ſub-tracted, the proper particles muſt ceaſe to be in equilibrio. A ſtep toward aggregation will immediately take place, and the equilibrium will be reſtored. A further ſubtraction will pro-duce a further ſtep toward aggregation, and a conſequent equilibrium; and theſe operations will be repeated ſo long as the cauſe of ſubtraction continues, and the longer its duration the larger will be the reſulting cryſtalline maſs. If the above mode of reaſoning be admitted, it will ſuffice to apply the laws of equilibrium to deduce the laws of cryſtalline forms. The laws of equilibrium to which I allude, are thoſe of the equilibrium of fluids, with certain modifications which ſhall hereafter be explained. According to theſe laws, that the preceding conditions may take place in the formation of a cryſtal, it will be neceſſary that they take place in the forma-tion of each and every part of it, whatever may be the figure or the ſmallneſs of thoſe parts. They muſt alſo take place in thoſe laſt cryſtals which contain the leaſt poſſible number of particles; and as theſe particles are in equilibrio, and in the greateſt poſſible ſtate of proximity to each other which cir-cumſtances will permit, it muſt follow, to fulfil all the condi-tions, that theſe particles form a ſymmetrical polyëdron. This peculiar diſpoſition of the cryſtalline particles conſtitutes the modification, to which I alluded, in the laws of the equili-brium of fluids; it being neceſſary in this caſe to take the num-ber of cryſtalline particles into account, which is not the caſe when treating of the particles of a fluid. In a fluid, the par-ticles and their reciprocal diſtances are ſuppoſed infinitely ſmall; but the cryſtalline particles and their diſtances to each

other *

other muft be fuppofed finite. This material difference will necessarily cause a difference between the forms of their aggregates. Thofe formed with the particles of a fluid will be bounded by curved lines; the cryftalline aggregates, on the contrary, will be terminated by ftraight lines; and when thefe ftraight lines are not too fmall, the boundaries will be fenfibly rectilinear.

To afcertain what the power is that holds the particles in the ftate of reft, though not in clofe contact, is not the queftion; but the form of the polyëdrons which they produce. The clofer adhefion of the particles to be obtained by the fubtraction of caloric, fufficiently demonftrates that the particles are not in clofe contact with each other, and the conftancy of the cryftalline forms equally proves that they are in equilibrio. We will now proceed to the conftruction of a cryftal with thefe cryftalline particles. That the conftancy of the form in the large cryftal be preferved, the particles muft be in equilibrio. That the equilibrium be preferved, the forces that folicit the particles to motion muft mutually deftroy each other. That the mutual deftruction of thofe forces be effected, thefe forces after having been decompofed into other relatively parallel to three axes perpendicular to each other, and having a common point of interfection, muft each meet in its direction another force equal and diametrically oppofed to it. This will be obtained if the fimilar particles are arranged on ftraight lines parallel two and two at equal oppofite diftances from the common centre, and bifected by lines paffing through that centre; but if the particles are thus arranged, they muft produce fymmetrical folids bounded by planes; and they are thus arranged: for if a foreign force, an excefs of caloric for example, does not impede the free arrangement of the particles in the formation of the cryftal, their exterior difpofition will follow as much as poffible their interior arrangement; but their interior arrangement muft be on ftraight lines, or the cryftal would ceafe to be homogeneous; their exterior difpofition will therefore be on ftraight lines.

As the circumftances giving rife to the approach of the particles may be in the higheft degree variable, it muft follow that the forms produced may be diverfified in the extreme. Such, Sir, is the anfwer I fhould fubmit for the folution of the queftion propofed.

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When fpeaking of the approach of the proper particles, I faid that it might be occafioned by the fubtraction of certain interpofed particles which obftructed the approach of the proper particles. The former are generally water, caloric, or any fluid elaftic or not. Their exit may perhaps make place for others, fuch as light, electricity, &c. &c. But the effential point is, that whatever thefe particles may be, they are in perfect equilibrio with the proper particles, otherwife they would become perturbing forces. Hence it follows, that not only the integrant particles of the cryftal, but all thofe that are mixed with them, the chemical or component particles and even the vacuities, muft follow the fame laws. It alfo follows, that if each fpecies of particle (even the chemical) that enters into the formation of the cryftal be feparately confidered, each fpecies will have its diftinct fymmetrical and polyedral form. The forms will penetrate each other, while the particles will not only not penetrate, but not even touch each other. All forms would ftand in the fame predicament as the regular octaëdron, which contains, as the Abbé Haüy has demonftrated, fix regular octaëdrons and eight regular tetraëdrons, each tetraëdron containing one octaëdron and four tetraëdrons. It will further follow, if the chemical elements can be looked upon as particles which are not in contact with each other, that we may from thence mathematically determine chemical affinities.

I have now, Sir, but one tafk left; to fpeak of the application our author has made of algebra and geometry to cryftallography. Many perfons complain of the difficulty neceffarily refulting from it in the ftudy of mineralogy ; and dare not engage in it, uncertain whether they will find a compenfation for their trouble. Our author has therefore adopted a double plan, and begins by expofing his theory by a feries of reafonings and arguments which will fuffice to make the reader underftand it, or any difcoveries made in confequence of it. He then expofes the theory in the moft correct of all languages —mathematical analyfis; by far the moft interefting, and the only means of making difcoveries one's-felf : and who can be callous to the pleafure of difcovering an unknown truth ? If the folution of a problem gives fo much fatisfaction, though the data be only imaginary, what muft be the fenfations of thofe who are happy enough to folve problems whofe data are

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fet by *Him* whom the greateſt of pagan philoſophers calls the *Eternal Geometrician?* This recalls reflections to my mind which I cannot ſupprefs. Converſing one day with the Abbé Hauy, he was taking a curſory view of all the modern diſco- veries; when he could not help remarking, that there was not one of them but what furniſhed victorious arms to the cauſe of religion. My anſwer was, that in future the name of God would be as diſtinctly written on a cryſtal as it had hitherto been in the heavens. The obſervation of this moſt religious and ingenious man reminds me of the ſaying of lord Bacon : " A little philoſophy eſtranges us from religion, but a great deal reclaims us again." Even d'Alembert could not help ſaying, " An atheiſt in the Carteſian ſyſtem is a philo- ſopher miſtaken in the principles; but an atheiſt in the New- tonian ſyſtem is ſomething worſe, an inconſequent philoſo- pher."

But to return to the mathematical part of our author's the- ory : the branch of mathematics, and the manner in which he treats it, are almoſt new. The theory of polyëdrons had been nearly neglected by geometers, both on account of the diffi- culty to repreſent a polyëdron on a plane, and becauſe they did not feel the utility of the purſuit. Nevertheleſs, ſtrange to ſay, all the regular figures that are to be found in one of the three kingdoms of nature are polyëdrons. In this point of view, the branch of mathematics illuſtrated by the Abbé be- comes very intereſting; and it is not a little ſo, to ſee with what ingenuity he extricates himſelf from the difficulties he meets with in his reſearches. He forms all the polyëdrons, however complicated, of little equal rhomboids or parallelo- pipedons, and by that means he reduces the theories of every poſſible polyëdron to that of the rhomboid, which is extremely ſimplified by two very ſimple remarks : 1ſt, That in all equi- lateral rhomboids, whatever may be the ſpecies, their projec- tion on a plane perpendicular to their axes will always be a regular hexagon : 2dly, That the axes will always be triſected by perpendiculars drawn from all the lateral ſolid angles. His theory has alſo led him to diſcover in a variety of cryſtals geometrical properties, which muſt be highly gratifying to geometers. But the great advantage to be derived from it is, that it enables us with the feweſt poſſible data to calculate the cryſtalline forms juſt as aſtronomers do the motions of the heavens.

heavens. By the very means by which the latter determine the future motions of the heavens, the Abbé decides which forms are poffible and which are impoffible. It is thus by his fimple and general law of cryftallization, " the number of the ranges of the fubtractive particles muft always be a commenfurable quantity," that he has demonftrated the regular dodecaëdron and the regular icofaëdron to be impoffible forms in mineralogy. As the immortal Newton, by having difcovered the law of attraction to be " in the inverfe ratio of the fquares of the diftances," explained and calculated every thing in the vaft regions of the firmament; fo at the other extremity of the creation the Abbé Haüy, by means of a fingle law which he has difcovered, explains the irregularities and calculates thofe problematic formations with which the mineral kingdom had hitherto aftoniflied the natural philofopher.

Laws, Sir, that refult from the ftudy of nature, enjoy this ineftimable advantage, that they always lead to *equations*; and it is only by the help of equations (expreffed or underftood) that queftions can be folved which relate to objects that can be either counted or meafured.

Of late, Sir, the word *nature* has been fo much abufed, that I muft beg leave to ftate the precife fenfe in which I wifh to be underftood whenever I made ufe of that word in the courfe of this letter. The Abbé Haüy found it neceffary to take a fimilar precaution at the beginning of the excellent work *(Traité de Phyfique)* he has lately publiflied. He fays: " This word NATURE, fo frequently in our mouths, can only be looked upon as an abridged expreffion, either for the refult of thofe laws which the GREAT CREATOR has imprinted on the univerfe, or for that aggregate of beings the works of his hands. Nature, thus viewed in its true light, is no longer a fubject of cold and fterile fpeculation. The ftudy of its productions, of its phænomena, ceafes to be a mere exercife of the mind; it moves the heart, and ftrengthens the moral virtues in man, by awakening in his mind fentiments of refpect and admiration at the fight of fo many wonders bearing the vifible characters of infinite power and wifdom."

With thefe fentiments I remain, Sir, your's,

A. Q. BUEE.

July 13, 1804.

Obfervations

IV.

Obſervations on Mr. GOUGH's *Strictures on the Doctrine of Mixed Gaſes, &c. In a Letter from Mr.* J. DALTON.

To Mr. NICHOLSON.

SIR,

FROM the formidable manner in which Mr. Gough opens Introduction. the campaign, I might expect him to bring a hoſt of *facts* and *arguments* to his aſſiſtance; the *facts,* however, he prudently keeps in reſerve, and it is to be feared ſeveral of the *arguments* too, as thoſe already drawn out are ſcarcely of force ſufficient to provoke reſiſtance.

Mr. Gough's firſt *argument* is, that the ſubject in diſpute is more properly denominated *hypotheſis* than *theory.* This is certainly not worth contending about.

Mr. Gough's next *argument* is, that the hypotheſis is not mechanical; but as this cannot be made good, it ſeems, without a geometrical diſquiſition of length much exceeding an ordinary letter, the philoſophical world are to wait till the ſame ſhall appear in due courſe. I might therefore, for the preſent, wave any remarks on this head, as a long and equally abſtruſe defence might be expected. But as I think Whether the geometry has nothing to do in the buſineſs, and that all that hypotheſis be mechanical. can be ſaid effectually, either for or againſt the hypotheſis, being conſiſtent with mechanical principles, may be comprized in one ſhort paragraph, I ſhall diſcuſs the argument here.

Oxigen repels oxigen, but not azote. This is a poſtula- Poſtulate. The tum; and being admitted, it follows, that if a meaſure of parts of a gas repel each other, oxigen be put to one of azote, the oxigen finding it porous, but not thoſe of muſt enter the pores, and *vice verſa,* till the two gaſes ſeve- other gaſes. rally making their way into the interſtices of the other, at laſt obtain a perfect equilibrium, and then preſs with equal force on all the ſurrounding bodies, and no longer preſs on each other. This is ſo plain and obvious an inference, and ſo little involves any mechanical conſideration, that I ſhould have juſtly incurred blame for inſulting my readers with the appearance of mathematical demonſtration in the caſe. As well might I have attempted, from the elements of Euclid,

to

to demonftrate to a cottager, that if he put a fieve over his chimney the fmoke would ftill efcape, though interruptedly; or to a chemift, that if he drilled holes in an exhaufted receiver, it would in time be completely filled with air.

Mr. Gough next remarks upon my opinion, that the atmofphere of aqueous vapour is fufficient to prevent the ocean from efcaping into the air, which he feems to think wonderful: Upon this I may remind him of another wonderful fact, which I confider the fame in effect; that is, the preffure of like vapour on a cup of water in an exhaufted receiver, prevents the water from efcaping out of the cup. When Mr. Gough fhall explain how this fact is to be accounted for, I may avail myfelf of his explanation to apply to the other. He proceeds, however, to demonftrate the impoffibility of an aqueous atmofphere; but inftead of that, he demonftrates *the impoffibility of an atmofphere of any kind preffing on water, without at the fame time forcing the water up into its pores.* Luckily, as Mr. Gough obferves, the facts do not countenance the conclufion; and therefore, however rigid the demonftration, it fhews that the previous data are not correctly affumed. The confideration of this fubject is, notwithftanding, an important one; it is more than a year fince I urged Mr. Gough to pay attention to it, and to attempt a folution of the difficulty which perplexes all theories of the atmofphere alike: I did it the rather, becaufe I thought him well qualified for a fubject of this nature, where the aid of mathematical fcience muft be of fubfervience. The fubject has never been at all explained that I know of. I have made an attempt which has not yet been publifhed, except in a lecture laft winter; but I fhall ftill be glad to avail myfelf of any affiftance I can obtain.

Suppofing that in the loweft ftratum of an atmofphere incumbent on water, there is *one* particle of gas for *one hundred* of water, (which may be the cafe, confidering their relative denfities as *one* to a *thoufand*); *query,* How does the air diffufe its preffure over all the hundred particles equally, in fuch fort that no column of particles of water is forced up into the interftices of the atmofphere by the inequality of the preffure? If Mr. Gough will explain this, either for his new theory or for any other, I will engage to remove all difficulty on this head which attaches to my hypothefis.

Mr.

Mr. Gough next wants a rigorous proof of the propofition, that one gas affords an impediment to the motion of another, but that two fuch fluids finally overcome their mutual obftructions, and occupy the fame fpace in a ftate of perfect independence.   No one acquainted with the experimental part of pneumatic chemiftry would have required proofs of fuch facts, becaufe he daily experiences them.   Take two phials filled with different gafes; apply the mouth of one to that of the other for a few moments; upon withdrawing it, the two phials will be found to have interchanged very little of their contents.   This is a moft unqueftionable proof that gafes afford impediment to each other's motion; for, into a perfect vacuum the air rufhes inftantaneoufly.   Again, let the phials remain in connection for a few minutes, or at moft, hours, and they will be found to have both gafes in the fame proportion; and this ftate will continue in perpetuity afterwards.   That they are ultimately *independent* on each other, is fufficiently marked by the circumftance, that any fubftance having an affinity for one of them will withdraw it from the mixture, if it will take it alone.

Impediments afforded by one gas to the diffufion of another.

Mr. Gough proceeds to give a *new theory of mixed gafes:* At the commencement of this controverfy he came forth to *defend* the all-fufficiency of the old doctrine of the chemical union of water and air, and the homogenity of the atmofphere, and to *attack* the new doctrine, which profcribes chemical union in thefe inftances, and places for its fundamental and diftinguifhing maxim, that " mixed gafes neither attract nor repel one another :" It was therefore with no fmall furprife that I found, upon his advances, that a *new theory* was ftill requifite; but this furpsife became aftonifhment, when I found the preamble admitted the exiftence of certain, " gafes that neither attract nor repel each other." The only clear information I could obtain from this fudden revolution was, that Mr. Gough ufes the terms *theory* and *hypothefis* in a contrary fenfe to what philofophers in general do.   When I brought my *hypothefis* (as he calls it) forward, it was fupported by an extenfive train of facts, the refult of long and careful inveftigation, none of which, that I know of, has fince been controverted; I mean what Mr. Gough alludes to as a " number of probabilities of an experimental nature :" Whereas Mr. Gough's *theory* is propofed without a fingle

Mr. Gough's new theory of mixed gafes.

a single fact to corroborate it, merely to try how far it may be found to agree with facts already known. Leaving Mr. Gough to develope his theory, which I confefs I do not comprehend, and therefore cannot follow him in the application, I proceed to his concluding remarks on the law of affinity, which connects water with the different gafes.

Water fufpended or diffufed alike in all gafes.

Sauffure, to whom Mr. Gough refers, in the ninth chapter of his fecond effay inftructs us, that the *folvent powers* of common air, carbonic acid, and hydrogen, are the fame as far as his experience goes. The refults of Kirwan's experiments I am unacquainted with. Clement and Deforme, in the forty-fecond volume of the *Annales de Chimie* for 1802, have clearly fhewn, that all gafes take up the fame quantities of water, alcohol, and ether, in like circumftances; and I think I have fhewn, that thefe quantities are precifely the fame as a torricelliam vacuum of the fame capacity takes up: So that unlefs Mr. Gough can hold out fome further encouragement to the refumption of the enquiry, it feems hardly likely that any one will undertake to inveftigate the *diverfities* in the law of affinity between gafes and water, when there does not appear a fingle fact that points out any affinity at all in the cafe.

I am your's, &c.

J. DALTON.

Manchefter, Sept. 8, 1804.

V.

Account of the Striking Part of an Eight-Day Clock. By Mr. JOHN PRIOR, *of Nefsfield, Yorkfhire.* Communicated to *the Society of Arts.* *

To CHARLES TAYLOR.

SIR,

New ftriking part of a clock.

I HAVE taken the liberty of fending to you a new ftriking-part for an eight-day clock, of my own invention and workmanfhip, which I finifhed laft April; * * * * * *. I beg

* Extracted from their Tranfactions, 1803. A reward of thirty guineas was given by the Society, who have a model of the fame.

leave

leave, to inform the Society of fome of the advantages arifing from this new invention.

Firft, it confifts in a wheel and fly, with fix turns of a fpiral line, cut upon the wheel, for the purpofe of counting the hours. The pins below this fpiral elevate the hammer, and thofe above are for the ufe of the detent.

This fingle wheel ferves the purpofe of count wheel, pin wheel, detent wheel, and the fly wheel, and has fix revolutions in ftriking the twelve hours.

Permit me to fuppofe a train of wheels and pinions, ufed in other ftriking parts, to be made without error, and that the wheels and pinions would turn each other without fhake or play. Allowing the above fuppofition to be true, which every mechanic knows it is not, my ftriking part will be found fix times fuperior to others, in ftriking the hours 1, 2, 5, 7, 10, 11, and twelve times, in ftriking 4, 6, 8, and eighteen times, in ftriking 3, 9, and 12.

I have defignedly made a defect in the model herewith fent, in ftriking 2 and 3 o'clock, to fhew that what I have now advanced will, upon the trial, be found to be true.

.In ftriking 2, I have purpofely made an imperfection, equal to the fpace of three teeth of the wheel; and, in ftriking 3, an imperfection of nine or ten teeth; and yet both thefe hours are ftruck perfectly correct.

The flys in clocks turn round, at a mean, about fixty times for every knock of the hammer, but mine turns round only three times for the fame purpofe; and fuppofe the pivots were of equal diameters, the influence of oil on them would be as the number of revolutions in each. *

It would be better for clocks if they gave no warning at all, but the fnail-piece to raife a weight fomewhat fimilar to the model now fent for the infpection of your refpectable Society.

Reference to Mr. PRIOR'S *Striking Part of his Clock.*
Plate VI. Fig. 1.

A. The large wheel, on the face of which are funk or cut the fix turns of a fpiral.

B. The fingle worm fcrew, which acts on the above wheel, and moves the fly C.

* See Cummings's Elements.

D.

D. The fpiral work of the wheel A. The black fpots fhow the grooves into which the detents drop, on ftriking the hour.

E. The groove into which the locking piece F drops, when it ftrikes one, and from which place it proceeds to the outward parts of the fpiral in the progreffive hours, being thrown out by a lifting-piece H, at each hour; the upper detent G being pumped off with the locking-piece F, from the pins in the wheel A.

In ftriking the hour of *twelve*, the locking-piece, having arrived at the outer fpiral at H, rifes up an inclined plane, and drops by its own weight to the inner circle, in which the hour *one* is to be ftruck, and proceeds on in a progreffive motion through the different hours till it comes again to *twelve*.

I. The hammer-work made in the common way, which is worked by thirteen pins, on the face of the fpiral.

Fig. 2.—K. The thirteen pins on the face of the fpiral, which work the hammer-work.

L. The outer pins, which lock the detent.

M. The pump-fpring to the detent.

VI.

On the Coft of making Phofphorus. In a Letter from J. P.

To Mr. NICHOLSON.

SIR,

Briftol, September 16, 1804.

HAVING lately been led to confider the various proceffes by which phofphorus might be obtained, with a view to determine the moft eligible, I thought that a very fhort fketch of what had occurred to me on the fubject, might be not improperly introduced in a fpare corner of your Journal; if you fhould be of that opinion, the contents of the following pages are much at your fervice, from a very refpectful admirer of your unremitting exertions in the caufe of Science and Philofophy.

J. P.

1. In

1. In the ufual method of procuring phofphorus from bone-dufl, and fulphuric acid, the value of the ingredients is very fmall, the labour of the procefs, with the fuel employed for the evaporation and diftillation form the principal confideration in eftimating the value of the product.

Eftimate of expence of obtaining phofphorus from bones. Refult 3s. per oz.

One pound of bones calcined and pulverized with half a pound of fulphuric acid, will produce, according to Fourcroy, about three quarters of an ounce of phofphorus; if the labour and fuel be eftimated at two fhillings, and it cannot be probably much lefs, the phofphorus would then coft the manufacturer about three fhillings an ounce.

2. If to the acid folution obtained by the above method be added 1¼lb. of acetite of lead, value three fhillings, the phofphate of lime will be decompofed, and two ounces of phofphorus fhould be produced from the precipitate,. (vide Fourcroy.) This appears then to be an improvement, fince 1¼oz. of phofphorus is procured for three fhillings. But perhaps the following procefs may be preferable to either.

The additional phofphorus obtained by adding acetite of lead, is rather cheaper.

3. To one pound of phofphate of foda, value two fhillings and fix-pence, add 1¼lb. of acetite of lead, value three fhillings; above one pound of phofphate of lead will be immediately precipitated, and about one pound of acetite of foda may be eafily obtained by evaporation. If this fhould be valued at two fhillings and fix-pence, the original coft of the ingredients will be reduced to three fhillings, add one fhilling for fuel and labour, and two ounces of phofphorus will be procured for four fhillings. It is needlefs to remark the great fuperiority of the laft method in point of facility, neatnefs and fimplicicy.

The procefs of decompofing phofph. of foda by acet. of lead affords phofphorus at 2s. per oz.

VII.

On the Purification of Water by Filtration; with the Defcription of a fimple and cheap Apparatus. In a Letter from SIR HENRY C. ENGLEFIELD, Bart. F. R. S. &c.

To Mr. NICHOLSON.

SIR,

I CANNOT but think the filtering machine of Profeffor Parrott, publifhed in your laft Journal, a very inconvenient form of a very common inftrument; but as the filtration of the water

Obf. on P. Parrott's filtering machine, &c.

water with which the metropolis is fupplied contributes materially to the health and comfort of thofe who ufe it, and in fact renders it purer than almoft any known fpring water, a very cheap and commodious apparatus for the purpofe may be confidered as an object of general utility. I therefore fend you a fection of a machine which I conftructed feveral years fince, and which any common carpenter can make for a very few fhillings, which may be thoroughly cleanfed at any time, and which occupies very little room.

As you have already honoured feveral of my fhort effays, publifhed in other collections, with infertion in your Journal, you will, I truft, not be difpleafed that we fhould become immediate correfpondents.

<div style="text-align:center">

I am, Sir,

Your obedient Servant,

H. C. ENGLEFIELD.

</div>

P. S. I will not anfwer for it that the machine I fend you has not been already introduced into ufe. If it has, you will of courfe fupprefs my letter; but if it has, Profeffor Parrot's filtre is quite nugatory.

Arrangement for purifying water on a large fcale.

A moft excellent arrangement for the purification of river water on a large fcale is mentioned in the writings of De Luc or De Sauffure, but I cannot turn to the paffage in their works. It was applied with complete fuccefs by the inventor, to the ftream which fupplied a large town in Switzerland. The machine (if it may be fo called) was as follows:

A is the upper furface of the ftream to be purified, B the bottom. A ciftern is funk of fix or feven feet in depth, and of a proper breadth, divided by parallel partitions, alternately rifing above the furface level of the ftream and open at the bottom, and level with the bed of the river, and clofed at the bottom. It is obvious that the courfe of the water muft be in the direction of the arrows, and in this repeated and flow afcent and defcent, all floating impurities will be left at the top, and the heavier mixtures will fubfide. The ciftern may be eafily cleanfed, either by taking out the partitions, if it is on a fmall fcale, or by fending perfons down between the walls, if it is built permanently for a great ftream. Perhaps, indeed, a box having the partitions filled half the way up with fand or gravel, may on this plan be the beft of all filtres for domeftic ufe,

Defcription

Defcription of the Filtering Apparatus, Plate V. *Fig.* 1.

A B The exterior tube, two feet high and fix inches fquare within.

C D The interior tube, four inches fquare at the top and three inches at the bottom. It reaches within about three inches of the bottom of the exterior tube, and is covered at the bottom with a coarfe linen tied round it. The ufe of this is to prevent the weight of the water from difturbing the fand. The upper end of this tube is formed into a funnel, for the convenience of filling it with water, and it refts on the outer tube. E a fpout for the exit of the filtered water.

Both tubes are filled with clean wafhed fand up to the dotted line juft below the fpout. A bag for ftopping the coarfer impurities may be adapted to the funnel. If this machine be placed under the cock of any common water ciftern, which is opened juft enough to fupply the funnel without running over, it will require no attendance, and will very feldom want cleaning. It is obvious that every part of the machine, when the two tubes are taken afunder is vifible to the eye, and eafily reached by the hand. The fand, when wafhed, will ferve many times.

If inftead of a funnel, a larger refervoir of water at the top is ufed, which may fometimes be convenient, it will be beft to fill the upper part of the inner tube for a few inches with clean fmall pebbles, as the pouring in water difturbs the upper furface of the fand.

It may be made either of wood or tin, but not of lead, for fear of impregnation. It is alfo evident that the rapidity of action of the filtre will be in a great degree regulated by the difference of level between the fpout and the furface of the water in the funnel, and by fupplying the funnel with a greater or leffer ftream, the machine may be made to act as quick or as flow as is wifhed.

As the water which fupplies the metropolis is often tainted with vegetable or animal fubftances putrified in it, it might be well worth while to try whether filling the inner tube with powdered charcoal might not tend to free the water filtred through it from the difagreeable tafte and fmell communicated by the caufes above-mentioned. It will alfo be advantageous to place the receiving veffel at fome diftance below the fpout, that the ftream may fall through as much air as it conveniently can.

VIII.

Experiments on the Effects of Heat modified by Compression, by SIR JAMES HALL, *Bart. Read in the Royal Society of Edinburgh, August* 30, 1804. *Communicated by the Author.*

Experiments of heat, modified by compression.

I BEG leave to announce to this Society the refult of a feries of experiments which have occupied my attention almost exclufively during feveral years. Thefe experiments relate to the effects of heat modified by compreffion, and are intended to inveftigate the peculiar and characteriftic principle of the Huttonian theory.

Dr. Hutton's theory of mineral compounds naturally produced in this way.

Dr. Hutton, in common with many former geologifts, has afcribed the formation of all mineral fubftances chiefly to fire. But, according to him, the influence of this element has been very much modified by compreffion, occafioned by the weight and ftrength of a vaft fuperincumbent mafs, which then preffed upon what is now the furface of our globe. In this manner he has anticipated the natural objection to all igneous theories which muft arife from a comparifon of various mineral fubftances with the products of fire in our furnaces; for he conceives that preffure by repreffing volatility would occafion the prefence, in high temperatures, of many fubftances which efcape in our fires, on a flight application of heat; and that thefe by their chemical relations would give rife to a ftate of things untried in any experiments hitherto publifhed, but fuch as to afford a fatisfactory explanation of all the natural phenomena upon his hypothefis, even of thofe which are the moft incompatible with the common action of fire.

The facts are that heat has fo acted in our globe; but the effects have not been fhewn by experiment.

The two fundamental *poftulata* required in this theory, namely, the action of heat, and the prefence of a fuperincumbent mafs are certainly allowable; fince the volcanos furnifh us with a proof that internal fire does act occafionally and in the irregular manner which this fyftem requires; and fince the fragmented and perturbed ftate of our ftrata enables us to fay with certainty that great changes have taken place, that enormous maffes have been removed, and that what was once placed at a great depth is now highly elevated.

But a third *poftulatum* is involved in this theory, which feems to be of more difficult admiffion. Granting that heat did act

on *

on fubftances conftrained by preffure, would its action be mo-
dified? Would that modification be fuch as is affumed in the
Huttonian theory?

To thefe' queftions Dr. Hutton has replied by arguments *Dr. H. has en-
founded on general analogy; and has refted the proof of his hy- *deavoured to
pothefis on its agreement with the phenomena of nature. In *establish thefe
that refpect few philofophical theories have been fo fortunate; *ological obfer-
for its univerfal application to every department of the mineral *vations.*
kingdom, and its folution of all the difficulties, afford a con-
currence of probabilities in its favour which prefs on the mind
with almoft irrefiftible conviction. Still it muft be owned that
the bafis of the fyftem is hypothetical; and a wifh has been ex-
preffed by every man of fcience who has attended to the fub-
ject, that this bafis fhould be fubmitted to the teft of experi-
ment.

My object has been to accomplifh that end, and to bring *The author re-
this great queftion to an *experimentum crucis.* By placing fub- *curs to actual
ftances in the predicament affigned to them in the Huttonian *experiment.*
theory, I have endeavoured to imitate the fuppofed procefs of
nature. In this attempt I have met with great and numerous
difficulties, but I have at laft fucceeded beyond my original
expectation, and have obtained refults, which, if I am not
greatly deceived, eftablifh as a law of chemiftry the moft pa-
radoxical of Dr. Hutton's pofitions.

My experiments fhew, that when pounded carbonate of *Carbonate of
lime, produced by the trituration of chalk, of marble, of the *lime agglutinates
fhell of a fifh, or of calcareous fpar, after being rammed into *wood) in ftrong
a fmall tube of porcelain, is expofed in veffels of fufficient *clofe veffels, and
ftrength and tightnefs to the heat of 21 or 22 of Wedgwood's *ftone.*
pyrometer (that is to the heat in which pure filver melts,*)
the

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* I take this opportunity of mentioning that a very material er-
ror feems to prevail with refpect to this point of the pyrometrical
fcale. The error is the more formidable that it has been introduced
and fanctioned by the higheft authority poffible in fuch a cafe; I
mean that of Mr. Wedgwood himfelf. In his account of the
pyrometer he gives a table, expreffing the effects produced at va-
rious points of temperature, and ftates 28 as the melting heat of
filver. Now it confifts with my knowledge that pure filver melts at
22. I learned the fact from Dr. Kennedy, and I have had occafion
to confirm the truth of it in numberlefs trials.

the carbonate fhrinks upon itfelf and agglutinates into a firm mafs, which in point of hardnefs and fpecific gravity approaches very near to common lime-ftone, and fometimes equals it, and which has frequently acquired the fparkling fra&ure, the femi-tranfparency, the fufceptibility of polifh, and the general afpe& of marble.   The fame refult is obtained when a folid piece of chalk is treated in a fimilar manner, and the chalk being previoufly meafured in Wedgwood's gage, is found to contra& during the a&ion of heat three times more than the pyrometer pieces do in the fame temperature.   During the a&ion of heat, the carbonate is found to have loft very little of its weight; that lofs amounting in many cafes to lefs than one per cent. and in fome experiments it has undergone no fenfible lofs at all, or fo very fmall a one, that it may be negle&ed without fear of error.   When thrown into an acid, this artificial limeftone effervefces violently as it diffolves, the difcharge of gas continuing whilft the fmalleft atom of carbonate remains vifible.

Great contrac-
tion of the
chalk.

I have been in poffeffion of this fa& fince the year 1801, and I long attempted in vain to carry the experiment farther, fo as to accomplifh the fufion of the carbonate.   In one folitary and accidental inftance, I had fucceeded in obtaining it in a ftate of real froth, which could not have been produced without previous liquefa&ion; but being unable to repeat this refult, I was unwilling to publifh it or any of the fa&s already ftated, till I could do fo in a more fatisfa&ory manner.   In the courfe of laft winter, with the help of many improvements in my mode of operation, and of ftronger apparatus, I at laft acquired the power of performing repeatedly and even with tolerable certainty,. what at firft had been the effe& of chance. In thefe experiments carbonate of lime has not only been ag-

Imperfe& fufion
of the chalk.

A&ual fufion.

This obfervation relates to the pyrometer pieces fold by the late Mr. Wedgwood, which were formed of a mixture of alumine with Cornifh porcelain clay.   This fet having been the only one ever diftributed amongft chemifts, muft certainly be looked upon as the ftandard.   Other fets had previoufly been made by him of Cornifh clay alone, which had never been fent abroad, or at leaft only given to fome friends.   It is poffible that the difcordance alluded to in this note, may have been occafioned by experiments made with thofe firft fets, which may have poffeffed different properties from thofe afterwards fold.

glutinated;

glutinated, but actually fufed; the fubftance finking upon itfelf with a round and gloffy furface, and exhibiting every proof of a vivid fluidity, fimilar to that of melting fealing wax. In general the fufion has been accompanied with a flight ebullition, which has fometimes changed the mafs to a kind of froth, and fometimes has merely produced fome fcattered air bubbles. The whole externally and in its fracture fhines much; this fhine, arifing in fome cafes from numberlefs facettes of cryftallization, and in others from a fmooth and continued glofs, like that of glafs. In many fpecimens the cryftallization of newly formed fpar is diftinctly vifible; the cryftalline mafs confifting of parallel plates, which reflect together with one glofs. Some of thefe are difcernible by the naked eye, though in general to fee them well we require the help of a lens. As foon as the carbonate becomes foft, it begins to act powerfully on the tube of porcelain (generally formed of pure Cornifh clay) in which it is confined; the compound fhewing itfelf to be much more fufible than the pure carbonate. It penetrates the minuteft crevices, and fpreads along the cup to a confiderable diftance from the point of contact between the carbonate and the tube; its termination being marked by a black line, the caufe of which I have not difcovered. Previous to this ftage of fufion no action whatever feems to take place between the carbonate and the porcelain, the former receiving from the latter an accurate impreffion of its fhape, acquired doubtlefs when the powder was rammed into the tube. In this cafe the carbonate remains quite loofe, and is often heard to rattle before the veffel is opened. Where pounded filex has been rammed into the tube in contact with the carbonate, an union has fometimes taken place, producing a fubftance having fomewhat the appearance of chalcedony, but which fhews evident proof of fufion, it having flowed fo as to form little ftalactites and ftalagmites. This fubftance effervefces feebly in acid, in fome cafes it leaves a femi-tranfparent cloud of undiffolved matter, in others diffolves entirely the folution, yielding a jelly when evaporated to a certain pitch. This affords proof of a real union between the carbonate and the filex.

In all the experiments alluded to in this paper, the veffels have been expofed to a violent expanfive force, by which a great number of them have been deftroyed, and the experiments have often been loft or refults obtained only of partial

Sparry cryftals in the fufed carbonate.

Action of the carbonate on the clay veffel.

Union of the carbonate and filex.

When the veffels have failed, the carbonate has fhewn different habitudes from lofs of acid.

3 fuccef.

fuccefs. But thefe have frequently been of value, by bfinging into view important collateral facts. Thus I have found that under certain circumftances, a partial calcination has taken place by the feparation of fome of the carbonic acid from the lime, though enough ftill remained to preferve many of the leading properties of a carbonate. When a lofs is fuftained, amounting only to two or three, or even four per cent. I find the fubftance ftill fufceptible of agglutination and fufion, but

its fufibility is greatly diminifhed, a heat of 40 or 50 being required to accomplifh what would have been done in 22 or 25, had the earth continued to be completely faturated with carbonic acid, and the carbonate thus obtained is apt to fall to decay by attracting moifture from the air. Thefe differences afford a good illuftration of the influence exerted by the acid as a flux on the earth.

Having thus afcertained the fufibility of the carbonate under preffure of indefinite amount, I became dubious of affigning its limits, and of difcovering the leaft force neceffary for this purpofe. In this view, in addition to my other devices, I followed thofe ufed by Count Rumford in trying the explofive ftrength of gun-powder.*

By means of a great weight preffing upon a fmall opening, and regulated by a counterpoife adjufted at pleafure, I was able to conftrain the carbonate to any given amount. In this manner I found that the preffure of 80 atmofpheres, anfwering nearly to half a mile of fea in depth, was requifite to produce

any effect of compreffion on the carbonate of lime, and that to execute the bufinefs well required a force four or five times greater.

I have likewife made fome experiments with coal treated in the fame manner as the carbonate of lime, but I have found it much lefs tractable, for the bitumen, when heat is applied to it, tends to efcape by its fimple elafticity, whereas the carbonic acid in marble is in part retained by the chemical force of quick lime. I fucceeded, however in conftraining the bituminous matter of the coal to a certain degree in red heats, fo as to bring the fubftance into a complete fufion, and to retain its faculty of burning with flame. But I could not accomplifh this in heats capable of agglutinating the carbonate; for I have found, where I rammed them fucceffively into the fame tube,

* Philof. Journal, quarto feries, I. 459.   •

and

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and where the veffel has withftood the expanfive force, that the carbonate has been agglutinated into a good lime-ftone, but that the coal has loft about half its weight, together with its power of giving flame when burnt, remaining in a very com- pact ftate with a fhining fracture. Although this experiment has not afforded the defired refult, it anfwers another purpofe admirably well. It is known that where a bed of coal is croffed by a dyke of Whinftone, the coal is found in a peculiar ftate in the immediate neighbourhood of the Whin, the fubftance in fuch places being incapable of giving flame, it is diftinguifhed by the name of blind coal. Dr. Hutton has explained this fact by fuppofing that the bituminous matter of the coal has been driven by the local heat of the Whin into places of lefs intenfity, where it would probably be retained by diftillation. Yet the whole muft have been carried on under the action of a preffure capable of conftraining the carbonic acid of the calca- reous fpar which occurs frequently in fuch rocks. In the laft mentioned experiment, we have a perfect reprefentation of the natural fact fince the coal has loft its petroleum, whilft the chalk in contact with it has retained its carbonic acid.

I have made fome experiments of the fame kind with ve- getable and animal fubftances. I found their volatility much greater than that of coal, and I was compelled with them to work in heats below rednefs; for even in the loweft red heat they were apt to deftroy the apparatus. The animal fubftance I commonly ufed was horn, and the vegetable faw-duft of fir. The horn was incomparably the moft fufible and volatile of the two. In a very flight heat it was converted into a yellow red fubftance like oil, which penetrated the clay tubes through and through. In thefe experiments I therefore made ufe of tubes of glafs. It was only after a confiderable portion of the fub- ftance had been feparated from the mafs that the remainder af- fumed the clear black peculiar to coal. In this way I obtained coal, both from faw-duft and from horn, which yielded a bright flame in burning.

The mixture of the two produced a fubftance having exactly the fmell of foot or coal tar. I am therefore ftrongly inclined to believe that animal fubftance, as well as vegetable, has con- tributed towards the formation of our bituminous ftrata. This feems to confirm an opinion advanced by Mr. Keir, which has been mentioned to me fince I made this experiment. I con- ceive

ceive that the coal which now remains in the world is but a small portion of the organic matter originally depofited, the moft volatile parts having been driven off by the action of heat before the temperature had rifen high enough to bring the furrounding fubftance into fufion, fo as to confine the elaftic fluids and fubject them to compreffion.

In feveral of thefe experiments, I found that when the preffure was not great, when equal, for inflance, only to 80 atmofpheres, that the horn employed was diffipated entirely, the glafs tube which had contained it being left almoft clean, yet undoubtedly if expofed to heat without compreffion, and protected from the contact of the atmofphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility.   Here then it would feem as if the moderate preffure, by keeping the elements of the fubftance together, had promoted the general volatility, without being ftrong enough to refift that expanfive force, and thus, that the whole had efcaped. This refult, which I fhould certainly not have forefeen in theory, may perhaps account for the abfence of coal in fituations where its prefence might be expected on principles of general analogy.

I have fhewn feveral fpecimens of thefe refults to my friends, in particular to Lord Webb Seymour, Mr. Playfair, and Mr. Davy, who have agreed in thinking that the inveftigation is now brought to fuch a ftage of advancement, that the refult ought to be made public.

I propofe in the courfe of next winter to lay before the Society a particular account of all thefe refults, and of the methods followed in obtaining them.   In the mean time I fhall now fubmit a few of them to the infpection of the gentlemen prefent.

Nos. 1, 2, 3, 4, 5, 6 and 7, were all produced in feparate experiments from pounded carbonate of lime.   No. 1, was amongft the firft of my fuccefsful refults, having been obtained in 1799.   It is a firm ftone, requiring a fmart blow of a hammer to break it.   It was inclofed in a cartridge of paper, the mark of which it ftill bears.   The other fix are ftill harder and more compact, approaching nearly in thefe qualities to common lime-ftone.   Nos. 2, 4, and 7, poffefs a degree of femitranfparency moft remarkable in No. 4, and all of thefe fpecimens exhibit an uneven fracture, approaching to that of bees

wax and marble. Their colours are varioufly though flightly tinged with yellow and blue; in particular No. 3, which though produced from common white chalk, refembles a yellow marble. Nos. 3, 5 and 6 have taken a tolerable polifh. No. 7 contains a fhell introduced along with the pounded chalk, and now clofely incorporated with it.

Nos. 8, 9, 10, 11, all formed from pieces of chalk expofed unbroken to heat and preffure. No. 8 is remarkable for a fhining grain and femi-tranfparency. Nos. 9 and 10 fhew parallel planes like internal ftratification which has often appeared in chalk, in confequence of the action of heat, though nothing of the kind could be feen in the native mafs. No. 11, very compact, and of a yellow colour.

By various trials, to be given in detail hereafter, it appears that the carbonate in all thefe experiments has undergone a great diminution of bulk, amounting in fome cafes to more than ⅓ of the original mafs; and that its denfity has been proportionably increafed. At the fame time the porofity of the fubftance has diminifhed in a ftill higher degree. Thus it is found that chalk in its natural ftate abforbs and retains from 20 to 25 per cent. of water; but after being expofed to heat under compreffion, that it does not abforb quite 0.2 per cent. or the 500 part of its weight.

Nos. 12, 13. Examples of welding, in which the pounded chalk has been incorporated with a lump of chalk, upon which it had been rammed, fo that their joining is hardly vifible in the fracture.

Nos. 14, 15, 16. Shewing the fufion of the carbonate well advanced, with a confiderable action on the porcelain tube. In No. 15, the rod of chalk is half melted, and a yellow fubftance produced by a mixture of the carbonate with the porcelain. No. 16 is a lump of chalk, in a ftate indicating foftnefs; a piece of porcelain, which lay in contact with it, having funk a little into the fubftance of the carbonate.

Nos. 17 and 18, being delicate, are inclofed in tubes of glafs. No. 17, formed from pounded chalk, fhews in one part the moft complete formation of fpar with its rhomboidal fracture I have ever obtained. The carbonate having loft fome of its carbonic acid, had crumbled fo much in its effential parts by the action of the air, that the cryftallization was no longer vifible, and I had given up the fpecimen for loft till within thefe

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Other specimens
of bodies ex-
posed to heat
under strong
preffure.

these few days. When employed in examining these refults, a mafs of the carbonate broke in two, and exhibited the fracture now before us nearly in as good a ftate as it was originally. I immediately inclofed it in a glafs tube, and fealed it up with wax, fo that I have hopes of preferving it. In the mean time I am happy to fhew it entire to the Society. No. 18, likewife from pounded chalk is perfectly frefh and entire, though made more than a year ago; it fhews fome beautiful clear cryftals of fpar in parallel plates, but is fo fmall as to require the ufe of a glafs.

Nos. 19, 20, 21, fhew examples of fufion and action on the tubes. In number 19, a fhell is finely united to fome pounded chalk. In No. 20, the mafs originally of pounded chalk is finking upon itfelf, and acting at the fame time upon the tube. The pure carbonate in its fracture fhewing brilliant facettes of cryftallization. In No. 21, the carbonate in a ftate like the laft; the compound of porcelain and carbonate fhewing its liquidity by penetrating the tube fo as to form a diftinct vein, and then fpreading on its outfide to a confiderable extent, terminating with the black line above alluded to.

Nos. 22, 23, 24, give proofs of entire fufion. In No. 22, we fee two porcelain tubes inclofed for prefervation in a glafs tube, the fealed end of which muft be held downwards, to fhew the pofition in which the experiment was made. The innermoft porcelain tube ftands with its muzzle upwards, and the outermoft covers it in the inverfe pofition; the carbonate was contained in the inner tube. During the action of heat, the barrel failed fuddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almoft to its bottom; thus proving that immediately previous to the failure of the apparatus, the carbonate had been in a liquid ftate. No. 23, two maffes of carbonate, welded together in a complete ftate of froth. The fubftance fhining and tranfparent. No. 24, two feparate maffes expofed together to heat; one from pounded chalk, now in a ftate quite like the laft; the other put in as a lump of chalk dreffed flat at both ends, and a letter cut on each end (as done in many of the experiments.) It is in a fhining and almoft tranfparent ftate; at one end the flat form and the letter are ftill vifible; the other end is completely rounded in fufion, with a gloffy furface.

Nos.

Nos. 25, 26, refults of coal. No. 25, produced by the fufion of common coal under preffure in low red heat. It gave flame powerfully. No. 26, coal produced from horn. It is a fhining black fubflance, exactly refembling pitch or petroleum, and burns with a bright flame.

<div align="right">JAMES HALL.</div>

<div align="center">

IX.

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Atmofpherical Air not a mechanical Mixture of the Oxigenous and Azotic Gafes, demonftrated from the Specific Gravities of thefe Fluids. In a Letter from Mr. JOHN GOUGH.

<div align="center">

To Mr. NICHOLSON.

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SIR,

I HAVE already attempted to prove common air not to be a mechanical mixture. The arguments which I ufed for the purpofe, were drawn from the properties of refracted light and the motion of founds through elaftic fluids. Such proofs are of an indirect nature; and though the mathematician may fee the force of them, they may not carry an equal degree of conviction to the chemift, who ought alfo to be convinced. The following obfervations apply more immediately to the fubject; and I do not perceive how they can be refuted, unlefs this be done by difputing the accuracy of the experiments on which they are founded. The prefent enquiry has the recommendation of being ftatical, and the data of my calculations are borrowed from certain experiments made by Mr. Kirwan and M. Lavoifier. According to the former gentleman, if the weight * of atmofpherical air be denoted by 1000, that of an equal bulk of carbonic acid gas will be 1500, of oxigenous gas 1103, of azotic gas 985. An attempt to difcover the comparative weights of a number of gafes is a difficult undertaking; but the preceding ratios may be ufed with fome degree of confidence, becaufe they have been eftablifhed by a philofopher of the higheft reputation. Then in order to apply them to the bufinefs in hand, we will fuppofe the weight of 100 cubic inches of common air to be denoted by unity; though the real weight of the quantity may be ftated at 31

Introductory remarks.

Experimental data.

Equations to find the oxigen in air.

<div align="center">

* Effay on Phlogifton.

grains,

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grains, according to Mr. Kirwan. Alfo let the charaters w, x, and y, exprefs refpectively the cubic inches of the carbonic acid, the oxigenous, and azotic gafes, which compofe the mixture contained in 100 folid inches. Now $w + x + y = 100$ inches by hypothefis. But if 100 inches of air be denoted by unity, the fame bulk of carbonic acid gas will be expreffed by the fraction $\frac{1500}{1000}$, by the experimental data; confequently the weight of this gas in the mixture will be found by the following proportion; i. e. as $100 : \frac{1500}{1000} :: w : \frac{1500w}{100000}$. In like manner, the weight of the oxigen is found to be $\frac{1103x}{100000}$, and that of the azote is $\frac{985y}{100000}$. Now the fum of thefe weights is equal to unity by the premifes of the calculation : if then the equation be multiplied by 100000 on the common denominator of the unknown quantities, it affumes the following form ; i. e. $1500w + 1103x + 985y = 100000$. Thus it appears that we have only two equations, when the quantities to be determined are three ; this circumftance leaves the problem unlimited ; that is, the value of one of the quantities muft be difcovered without the aid of calculation.

Air contains carbonic acid.

M. Chaptal takes no notice of the carbonic acid in his obfervations on the conftitution of common air ; which appears to be an overfight, at leaft in a general view of the fubject ; for various proceffes of nature as well as art conftantly difcharge this gafeous acid into the atmofphere, where its prefence is alfo indicated by quick-lime being converted into a carbonate, when placed in open fituations. Thefe facts amount to more than a probability, that the lower parts of the atmofphere contain a flight admixture of the carbonic acid ; this fmall portion, however, was undoubtedly retained, in a great meafure, by the azotic gas which Mr. Kirwan ufed in his experiments ; becaufe he prepared it from common air, which was confined over mercury, together with a pafte of fulphur and the filings of iron.

x and y determined.

This fingle confideration induces me to follow the example of M. Chaptal in making w of no value ; and the ftep may be taken with the greateft fecurity in the prefent inftance, becaufe it increafes the value of x, if it alter it at all ; that is, the amount of the oxigenous gas, as found by calculation, will exceed the truth, on the fuppofition that the carbonic acid remained in the azotic gas, which Mr. Kirwan weighed. If, then, w be put equal to nothing, the preceding equations affume

fume the following forms; *i. e.* $x + y = 100$, and $1103\,x + 985\,y = 100000$. Multiply the former by 985, fubtract the product from the latter, and you will have the following equation; *i. e.* $118\,x = 1500$: Hence by divifion, $x = 12\tfrac{42}{59}$; and the excefs of 100 above this number gives $y = 87\tfrac{17}{59}$. The fteps of this procefs have been detailed minutely, with a view to enable the chemical reader, who has the leaft knowledge of algebra, to confider the grounds of the following conclufions, and to form his own judgment refpecting the weight of them.

It is evident from the foregoing calculation, that if 100 parts of a mixture of the oxigenous and azotic gafes, having the fpecific gravity of common air, were deprived of their oxigen in a graduated tube, the refiduum would meafure fomething more than 87 fuch parts, *i. e.* $87\tfrac{17}{59}$. But when an equal bulk of the atmofpherical fluid is treated in the fame manner, the portion of it which remains unabforbed, is much lefs than the preceding quantity; M. Chaptal makes it to be 72 fuch parts, and fome writers call it 78, the mean of which is 75. Now if we fubtract any one of thefe numbers from the refiduum fixed by calculation, a difference will be found, which cannot be referred to the unavoidable imperfections of eudiometers, becaufe it could hardly efcape obfervation in any inftrument of the kind, the leaft excefs being more than nine parts, or nearly a tenth, of the whole fcale of 100 parts. On the other hand, a mixture of 72 parts azote and 28 oxigen, or of 78 of the former and 22 of the latter, exceeds an equal bulk of common air in weight; confequently the atmofpherical fluid is not a mechanical mixture of the two gafes in queftion, if any credit be due to the experimental data.

Air not a mixture of oxigenous and azotic gafes.

Though air has been fhewn not to confift of the oxigenous and azotic gafes fimply mixed together, it is certainly a compound that may be refolved into thefe two principles: For befides fupporting refpiration and combuftion, it converts metals into oxides, and the nitrous gas into nitric acid; therefore it contains the oxigenating principle. On the other hand, when air is employed to oxigenate bodies, the refiduum of it is azotic gas of greater or lefs purity; confequently the atmofpherical fluid is a gafeous oxide of azote, which can be decompofed by art, though chemifts have not as yet difcovered a certain method of producing it at pleafure, by uniting the oxigenating matter to the azotic bafe.

Air a gafeous oxide of azote.

Facts

Statical analysis of air.   Facts have obliged me to give a name to common air, which has been hitherto exclufively applied to the dephlogifticated nitrous air of Dr. Prieftly. This remark being made for the fake of perfpicuity, I will endeavour, in the next place, to give a ftatical analyfis of the atmofpherical fluid. If 100 cubic inches of common air weigh 31 grains, the weight of an equal bulk of azotic gas will be 30.535 grains; becaufe as 1000 : 31 : : 985 : 30.535 : in like manner the weight of 100 cubic inches of oxigenous gas will be found to be 34.193 grains. The quantity of azo*c gas in 100 cubic inches of air, will be ftated at 75 inches in the prefent calculation, for the following reafons : *Firft*, Becaufe 75 is the mean of 72 and 78 ; *fecond*, Becaufe M. Lavoifier found, that four inches of oxigenous gas and 16 inches of air faturate equal quantities of the nitrous gas. Thefe premifes being fettled, we fhall find the weight of 75 inches of azotic gas to be 22.90125 grains; confequently the weight of the oxigen gas in 100 cubic inches of common air, is the excefs of 31 above the laft number, or 8.09875 grains ; therefore as 34.193 : 100 inches : : 8.09875 grains : 23.685 inches, which is the meafure of the oxigen gas in 100 cubic inches of common air, when the azotic gas is ftated at 75 inches. Thus it appears, that if 100 parts of the atmofpherical fluid were decompofed, the elementary gafes would occupy together no more than 98.685 fuch parts ; and a difference of a like nature will be obferved, if the azotic gas be called 72 or 78 *per cent.*; hence it follows that the denfity of air is lefs than that of the mechanical mixture of its elements. This pofition may appear paradoxical at the firft view, but chemiftry can furnifh various inftances of compounds, which are fpecifically lighter than the aggregates of their ingredients.

The oxigen of the air compared with the oxigenous gas.   This analyfis muft remain incomplete, until the powers of the oxigen of the atmofphere have been compared with the correfponding effect of an equal weight of the oxigenous gas. Such an attempt, however, is liable to great uncertainty ; becaufe the experiments which fhould fupply the neceffary data, are varioufly reprefented by different writers. M. Lavoifier fays, that four parts of oxigenous gas and nearly 16 of common air, oxigenate equal bulks of nitrous gas, namely 17,⅓ parts. On the contrary, M. Chaptal found by repeated experiments, that 12, or at moft 13, parts of air were fufficient

cient to faturate the fame quantity of nitrous gas. The data of the following calculation are taken partly from the one and partly from the other author: I have fuppofed with M. Lavoifier, that three inches of the oxigenous will acidify 13 inches of the nitrous gas: M. Chaptal is followed in other refpects, namely, the atmofphere is imagined to contain 72 *per cent.* of azote, and 39 inches of air are made equivalent to 52 of nitrous gas. The calculation, which is formed upon thefe fuppofitions, will in all probability prove incorrect; but I have ventured to infert it, as being a novelty, which is likely to excite enquiry. If 39 inches of air can faturate 52 of nitrous gas, 100 inches of the former require 133.333 of the latter. Again, if 13 inches of nitrous gas demand three of oxigen, 133.333 of the former will require 30.303 of the latter. But the weight of 30.303 inches of oxigen gas amounts to 10.361 grains, which is equivalent in effect to 100 inches of air. Now 72 inches of azotic gas weigh 21.985 grains, which being taken from 31 grains, leaves 9.015 grains for the weight of the oxigenous part of 100 inches of air; in round numbers, 90 grains of the latter kind of oxigen are equal in effect to 103 of the former.

I know that M. Lavoifier, in fpeaking of the conftitution of nitric acid, makes 100 grains of it contain 64 grains of the nitrous and 36 of the oxigenous gas; or 173 inches of the former and 105 of the latter. Had the data of the preceding calculation been taken from this ratio, the comparative fuperiority of atmofpherical oxigen would have been much greater than it appears to be by the laft paragraph. A preference, however, has been given to the preceding hypothefis, becaufe I defire to excite enquiry, rather than expect to eftablifh any thing of a permanent nature concerning the fubject.

The preceding arguments, in conjunction with others of a kind more uncertain than themfelves, fuggefted to me the probability of atmofpherical oxigen poffeffing a greater degree of efficacy than an equal weight of vital air. This idea occurred to me feveral months ago; and I at length refolved to propofe it to the public in a number of hypothetical queftions, which appeared in your Journal for Auguft. Thefe queries are evidently borrowed from fome ingenious fpeculations, that were publifhed in the eighth volume of the fame work *; for

The origin of the queries in the Phil. Jour. vol. viii. p. 246.

* Page 88.

the

the queries imply, in common with the remarks here alluded to, that the oxigenating matter is water chemically united to the pofitive power of the galvanic pile. In order to accommodate this notion to the cafe of common air; I have fuppofed that the oxigen of the atmofphere receives a ftronger charge of the power in queftion, than that which is imparted to vital air; in confequence of which it takes up an additional quantity of water, upon being difunited from the azotic bafis, and is thus converted into oxigenous gas. Should future experiments difcover that bodies, which are oxigenated by given portions of common air in contaȼt with water, acquire more weight than the air lofes, the difcovery will undoubtedly open a wide field for enquiry.

JOHN GOUGH.

Middlefhaw, Sept. 5, 1804.

X.

Letter from THOMAS YOUNG, *M. D. F. R. S. &c. announcing the Difcovery of a new moving Star, by Mr.* HARDING, *of Lilienthal; and on other Subjeȼts.*

To Mr. NICHOLSON.

DEAR SIR,

New planet lately difcovered.

I HAVE juft received a letter from Dr. Gaufs of Brunfwick, F. R. S. dated September 11, in which he informs me that, a few days before, Mr. Harding at Lilienthal had difcovered " a new moving ftar, moft probably another new planet of our folar fyftem." Dr. Gaufs is certainly a perfon on whofe judgment much dependance may be placed: He has fent fome further particulars of the difcovery to the Aftronomer Royal.

Maximum of denfity of water.

I take this opportunity of making a criticifm on a work of great experimental merit which has lately appeared. It relates to the apparent expanfion of water in a veffel of glafs. Mr. Leflie calculates, from the fuppofition that the expanfion of water is proportional to the fquare of the degrees of temperature above the freezing point, and from the expanfion of glafs meafured by General Roy, that the apparent maximum

of

of denfity fhould be at 6° of the decimal fcale: but he has taken in this calculation the mean rate of expanfion from the freezing to the boiling point, inftead of the expanfion at the boiling point, which, upon the fuppofition, is twice as great; hence the apparent maximum, on the fame grounds of calculation, fhould be at 3° of the decimal fcale, or at 37.4° of Fahrenheit. Mr. Dalton attempts to avoid this difficulty by fuppofing that the thin bulb of a thermometer expands more than the glafs tube employed by General Roy.

The fame ingenious author has made experiments on the elafticity of different fubftances, by meafuring the depreffion of the middle of a bar fupported at its extremities; but if I am not much miftaken, his inferences from them are by no means accurate. Mr. Leflie gives 671625 feet for the height of a column of deal equivalent to its elafticity; the true height refulting from his experiments, taking into confideration alfo the inequality of curvature, appears to me to be 4664000 feet; which is ftill little more than half as much as would be inferred from the experiments of Chladni on the longitudinal founds of fir wood. There muft have been fome inaccuracy in Mr. Leflie's experiments on fteel differently tempered: for it appears from the direct experiments of Coulomb on the flexure of bars, as well as from thofe of Chladni, and fome of my own, upon found, that the ultimate elafticity of fteel in fmall tenfions is the fame, whether it is harder or fofter. This may appear at firft fight paradoxical, but it admits of a fufficient explanation, which, together with many other illuf-trations of various parts of natural philofophy, will probably before long be laid before the public.

Your very obedient fervant,

THOMAS YOUNG.

Welbeck Street, Sep. 22, 1804,

Elafticity of bodies.

XI.

Description of an Instrument to ascertain the Number of Lifts made from a Mine, in any given Time. By Mr. JOHN ANTIS *.

SIR,

Inftrument for counting the lifts from a mine.

I HAVE lately been encouraged by fome gentlemen engaged in fome coal mines in this neighbourhood, to invent a machine, which would infallibly tell the number of boxes of coals drawn out of a pit in the courfe of a week, or any given time. I have juft now completed a model, which I think would anfwer the purpofe extremely well.

Before, however, I make it known, I thought I would inquire of you, whether fuch an invention might be of a more extenfive ufe, and as fuch would deferve the attention of the Society.

The machine is very fimple, and need not be expenfive; its properties are as follow :

1. It is of no confequence if the pit be ten, twenty, forty, or more yards deep. It will, notwithftanding this difference, only point out one box at once; and, fuppofing the pit to be feveral hundred yards deep, the principle will remain the fame, and the machine could be eafily adapted for it.

2. It is likewife of no confequence if the coals be drawn up by a hand windlafs, or any other machine turned by horfes or other powers.

3. No account is neceffary to be kept for a whole week, or any given time; as the machine can be calculated for any quantity of coals whatever, that can be drawn up in a given time. Only allowance muft be made for the number of perfons that go down and up in the fame way.

I have not yet heard of any contrivance of this kind; and therefore, if there be any fuch, it is unknown to me. Whoever knows the fraud, which is but too often committed by bankfmen, in accounting for the quantity of coals procured from a pit, will readily admit the utility of fuch a contrivance. And it may moft likely alfo be ufeful in tin, lead, and other

* From the Tranfactions of the Society of Arts, 1803.

mines.

mines.' Should the Society be of the fame opinion, then I will fend you a ftill more complete model than at prefent. Your fpeedy anfwer will very much oblige

<div align="center">Your moft obedient Servant,</div>

<div align="right">JOHN ANTIS.</div>

Fulneck, March 6, 1802.

Charles Taylor, Efq.

SIR,

I HEREWITH fend you a model of my machine, which I have before mentioned to you, for pointing out any number of boxes or bafkets of coals, or other minerals, which are drawn out of a pit in any given time. I fhall be much obliged to you, if you will lay it before the Society for the Encouragement of Arts, &c. for their infpection, and fhall be glad that they find it of public utility.

It will eafily be perceived, that by enlarging the wheels and multiplying the cogs of them, the number of yards required for the depth of a mine, as likewife the number of boxes which are wanted to be pointed out may be carried to any extent.

I am fo little acquainted with coal-pits, or any other mines, that I do not know the technical terms by which the different parts are called, nor all the methods now ufed to draw up thofe materials ; I muft therefore beg the indulgence of the Society, if I do not always exprefs myfelf with accuracy.

By inventing this machine, I aimed at afcertaining the number of boxes, my ideas refpecting which I will endeavour to defcribe, to the beft of my abilities.

Firft, the rope muft be fo long, that it can be faftened at its middle to the windlafs, and that each end thereof may reach to the bottom of the pit. This precaution will not be neceffary, if it can be prevented by any other means from fhifting or fliding, which I leave to others, more acquainted than I am with the prefent practice, to effect.

The roller A, *Plate* VII. *Fig.* 1, in the model, with the endlefs fcrew, reprefents the gudgeon in full fize of a hand windlafs, fuch as are ufed in this neighbourhood, particularly where the pits are of confiderable depth. As no great ftrefs is laid upon this fcrew, it may be made, in order to fave labour, of brafs,

<div align="center">I 2</div> <div align="right">and</div>

and driven, or otherwife faftened, upon any gudgeon. I
fuppofe the windlafs to be one yard in circumference; and
confequently forty revolutions of the windlafs are required
before the firft wheel B, upon which the brafs catch C is fixed,
turns once round. The pit, therefore, may be only ten yards,
or from feventy to feventy-five yards deep, yet it will make no
difference. All that is required to regulate the machine to the
depth of the pit, is as follows:—If the pit be little more or
lefs than ten yards, the firft wheel muft be fo placed in the end-
lefs fcrew, that the brafs catch C may be at the beginning, but
juft efcaped from the cogs of the fecond wheel D. It will
then only want five or fix revolutions of the windlafs backwards,
before the faid catch is ready to operate again. But fuppofing
the pit to be feventy yards deep, in this cafe, the catch C
muft be placed fo as nearly to be ready to act. By turning
backwards, the wheel may very nearly make two revolutions;
and as it can be ftill turned as far the other way, the catch will
only act once, provided the faid wheel does not quite make
two revolutions.

The model is about the fize required for a common hand
windlafs. The index will at once fhow how many times fifty
boxes, and how many above and below that number, have been
drawn up: not one of them can efcape. Allowance, however,
muft be made for the number of perfons who go up and down
in the fame way. Befides this, fince one box is always coming
up, whilft the other is going down, the numbers pointed at
muft be doubled, except a perfon was defirous to have a ma-
chine acting both ways, which would hardly be worth while.
As the machine is at prefent, though it points out a thoufand
boxes only, yet it will ferve for two thoufand.

The fame mechanifm, with very little variation, may be
fixed to a horfe machine, called a gin, in this neighbourhood;
viz. as a gin may be ten yards or more in circumference, the
action would be too flow, if moved by an endlefs fcrew; a
pinion, with four leaves, will be found much more convenient.
This pinion is fixed to the end of the upper gudgeon, which en-
ters the box containing the machinery. As fome accuracy is
required to make fuch a pinion work well, and thefe machines
are often coarfely conftructed, it will be neceffary only to make
about one inch and a half of the end of the faid gudgeon ex-
actly round, namely, that part of it which goes through the

bottom

bottom of the box, and then faften the latter fo, that it can give way to any remaining inaccuracy of the other parts of the gudgeon.

I have fent a drawing * of fuch a machine, which will be eafily underftood, fince the principle is the fame as in the model. But as gins are commonly made ufe of in deep mines, I have made it upon a larger fcale; viz. the firft wheel has fixty-four cogs; and this, with a pinion of four leaves, will require fixteen revolutions of the gin, for one of the wheel. Allowing ten yards for the circumference of the gin, it will make one hundred and fixty yards of rope; and confidering that the faid wheel can nearly make two revolutions, and that the catch will ftill act only once, it will confequently ferve for a pit of three hundred yards deep, as well as for one of ten or twenty.

In this machine, the three wheels are placed one on another, and the index is divided; viz. the fifty on the fecond, and the 1000 on the third wheel. As the wheels move, the two hands are ftanding, which will point out the numbers as exactly as thofe in the model. Indeed, machines for hand windlaffes may be made in the very fame way, and thereby be more fimple, though the difference is but fmall; viz. the box would require but one lid and lock, and the two fmaller wheels become unneceffary.

Though in fetting the machine fo as to begin counting with one, it may eafily be effected by lifting out the fpring, and then turning the wheel with the other hand, till both hands ftand upon one, yet, in the double index, the upper hand muft only be fet thus with the wheel, and the lower may be moved round on its focket.

In my humble opinion, my machines may be ufeful in two ways; firft, to the honeft banksman, as they will fave him the trouble of noting down each box which is drawn up, and prevent miftakes; fecondly, in the prevention of fraud, which will be their greateft and moft important ufe. To obtain this end, it is neceffary that the machine be fo contrived, that nothing effentially wanted can be difengaged. I have therefore made the model fo as to fruftrate any fuch defign. All is within the box; and neither the endlefs fcrew can be difen-

* This drawing is in the Society's poffeffion.

gaged

gaged from the wheel, nor the fpring; nor can the number on the index be altered without firft opening the box. This latter can be made of iron, if found neceffary, with a good lock to it; otherwife, the ftrefs is fo very little, that even one of wood, made in all refpects like the model, would anfwer, if it could be fecured from the effects of the weather. In the machine likewife, reprefented by the drawing, nothing can be altered, except the box be firft opened. This machine is faftened to the beam in which the upper gudgeon turns. To effect this, the bottom of the box muft have two projecting ends with holes in them, larger than neceffary to receive a ftrong pin or rivet; which pins muft have large heads, fo that when properly driven or riveted in, the box may be able to give way to any remaining inaccuracy in the lower parts of the gudgeon, as I before obferved. Thofe machines for hand windlaffes, like the model, are fixed to the fide of the poft into which one of the gudgeons turns; but perhaps thefe might likewife be contrived fo, that the gudgeon with the endlefs fcrew may-be a piece by itfelf, attached only to the windlafs by a fquare, or otherwife, as is often practifed in other machineries, fuch as cotton-mills, &c.

I am, Sir,

Your obedient Servant,

JOHN ANTIS.

Fulneck, May 18, 1802.

Charles Taylor, Efq.

Reference to the Model fent by Mr. ANTIS *to the Society.*

Plate VII. *Fig.* 1. A. The roller, with an endlefs fcrew, to be the fize of the gudgeon of a hand windlafs.

B. The firft wheel, of forty teeth, with a brafs catch C. It may be moved backwards or forwards by the endlefs fcrew.

D. A ratchet wheel with fifty teeth, which, when the wheel B is put in motion one way, the catch will flide over the teeth of it; but, on turning it the other way, the catch will drop into and forward this wheel one tooth, in which fituation it is prevented from returning by the fpring E, placed on the fide of the box which holds a tooth on the fide of the wheel.

F. A fmall pinion of four teeth, on the back part of the axle of the wheel D. This pinion moves a large wheel on the other fide of the box.

Fig.

Fig. 2.   G. The large wheel laft mentioned : it has eighty teeth.

H. A fmaller wheel of fifty teeth, fixed faft on the face of the wheel G ; which wheel H works a wheel I, of the fame number of teeth, fhown by dotted lines behind the figured dial-plate.

K. The fhort hand, or finger of the dial, is placed upon an arbor or collar of this wheel I, and moves with it.

The long finger, or hand of the dial-plate, goes upon a fquare on the end of the fpindle on which the above arbor moves; which fpindle moves forward this long finger one divifion in fifty of the outer circle marked on the dial-plate, every time the rope, or any thing attached to it, has been down to the bottom of the mine, and returned to the top.

The inner circle of the face of the dial is marked in divifions of fifty each. When therefore fifty draughts up and down have been made from the mine, the fhorter hand will be found, if both hands were originally fet correctly at the top, to have moved to the firft interior divifion marked 50; and fo on, in proportion, will advance as more draughts are made.

K, Is a board placed under the wheels B D, in *Fig.* 1, and which feparates them from the other wheel-work in *Fig.* 2, where only a few teeth of the wheel B appear behind it.

L L, Show the temporary handles in both figures; and the pofition of thofe letters denote that the gudgeon of the windlafs, when the machine is in actual ufe, fhould be there joined to, or make a part of the roller A.

Fig. 3, Shows, on an enlarged fcale, the form of the catch C. The fteel fpring M preffing againft the pin N, returns the catch to the tooth of the wheel D, when it has been forced back ; and a pin fixed underneath the catch moves in a groove O, made in the wheel B, to prevent the catch being preffed too far back, or thrown out improperly by the fpring.

A door is fixed on each fide the box, and fhould be locked, to prevent the hands being altered unknowingly.

The drawing of a machine for fimilar purpofes, intended to be ufed with a gin or horfe-wheel, alluded to in Mr. Antis's letter, is in the Society's poffeffion, if a reference thereto be thought neceffary.

XII.

On Galvanism.   In a Letter from RA. THICKNESSE, *Efq.*

To Mr. NICHOLSON.

SIR,   *Wigan, Sept.* 20, 1804.

On the caufes
which retard
difcovery.

ALTHOUGH the production of the electric fluid by the galvanic pile has never yet been fatisfactorily accounted for, it appears to me fomewhat eafy to be explained, from a confideration of the principles of chemiftry. This, I am aware, is a very bold opinion from a man who is a mere dabbler in philofophy; but as it muft be allowed that the ableft philofophical inquirers and experimentalifts have been guilty of extraordinary overfights, I truft my prefumption is excufable. When an ardent mind has once entered a wrong path in purfuit of knowledge, it is too intent upon the objects before it to turn afide, and too anxious to proceed to be induced to look back. Thus chemifts fifty years ago thought they obtained earth from water, becaufe they omitted to weigh the veffels in which they made their experiments; and thus, perhaps, the profeffors of galvanifm at the prefent day, being of opinion, as I believe they all are, that the electric fluid proceeds from the metals, devote little of their attention to the fluid employed in the pile, confidering it almoft as a mere conductor. Nothing can be more obvious to a chemift now than the neceffity of weighing the matter he fubjects to experiment, and alfo the product; yet the difcovery alone of this neceffity overturned the opinions, and falfified the. wifdom, of all the previous cultivators of the fcience, and led to the modern theory of chemiftry and its improvements; an omiffion how trifling in itfelf to keep men (philofophers too) poking in the dark for ages!

Argument or
inference that
water is com-
pofed of oxigen,
hidrogen, and
electricity.

Water, we are told, is compofed of certain parts of oxigen and hidrogen; but, as to form thefe bodies, from a ftate of gas at leaft, into water, it is neceffary to pafs an electric fpark through, or rather *into* them (no matter whether the procefs of formation be combuftion or not), and as water owes its fluidity to heat, the matter of heat and the electric fluid being probably the fame, or modifications of the fame body, I think it reafonable to affume that water is compofed of oxigen, hydrogen, and *the electric fluid*.

The

The experiments by which M. Volta and others have en- deavoured to fhew that piles compofed of metals only afford the galvanic fluid, are fo little demonftrative of it, that they feem to me equally illuftrative of the contrary; and I believe that all galvanic piles muft, with the intervention of fluids, be compofed of two metals, or *other fubftances*, which have an affinity, the one for oxigen and the other for hidrogen; for water and other fluids made ufe of in galvanic piles, are de-compofed; and not, I conceive, as hath generally been ima-gined, by the electricity of the metals; but by the action of affinities. For inftance, in a pile formed with zinc, copper, and water, the oxigen and hidrogen of the water having a ftronger affinity for the zinc and copper (the oxigen for the zinc and the hidrogen for the copper) than for each other, unite with them, the water being confequently decompofed, and the electric or galvanic fluid, which was contained in it, confequently fet at liberty.

In fupport of this theory I may alledge as facts, that the firft fhock from a galvanic pile is generally the ftrongeft, when the energy of the affinities is the greateft; that for a repeti-tion fome interval of time is requifite, whilft the decompofi-tion is taking place; that the metals in the pile act, not ac-cording to their quantities, but according to their furfaces— the greater thefe are, the greater being the quantity of water acted upon; that the zinc is always oxidated, and the copper (or filver) always acted upon by the hidrogen, being rendered more brittle, &c.; that any alteration in the arrangement of the pile, which brings two pieces of the fame metal to the fame ftratum of water, inftead of one piece of each metal (one to act on the oxigen and the other on the hidrogen), interrupts the procefs, and, no decompofition taking place, there is no electric fluid produced; and it may alfo be added, that this laft is a fact which cannot otherwife be accounted for.

I am not ignorant that a ftream of the galvanic fluid from the pile, paffed through water, decompofes it; but it is no proof, I prefume, that water contains none of this fluid, be-caufe it is decompofed by a greater quantity than will chemi-cally mix with it. If we pour a fmall quantity of water im-pregnated with carbonic acid gas into lime-water, the lime is precipitated, or *compofed*, the lime-water becoming turbid;

but

but if we add a further quantity, the lime is again decompofed, and the lime-water becomes again perfectly clear.

<div style="text-align:center">I am, Sir,</div>

<div style="text-align:center">Your moft obedient fervant,</div>

<div style="text-align:right">RA. THICKNESSE.</div>

<div style="text-align:center">XIII.</div>

Defcription of an Inftrument for drawing in true Perfpective from Nature, and of another of confiderable Simplicity and Cheapnefs for delineating Ovals. In a Letter from a Correfpondent, R. B.

<div style="text-align:center">To Mr. NICHOLSON.</div>

SIR,

Introduction. AS I obferve that you are willing, in your capacity of Journalift, to lay before the public any fketch or outline of invention that may promife to be ufeful, whether in its ultimate ftate of improvement or not, I am encouraged now and then to fend my thoughts, queries, obfervations, or news, as they may occur. The following inftruments are offered to your notice, in hopes they may appear in your excellent collection.

Inftrument for drawing in perfpective. *Fig.* 1, *Plate* VIII. is a fketch of an inftrument for perfpective, made fome years ago by Dolland, and of which I know not the inventor. A telefcope or camera is fufpended vertically on a frame by an univerfal joint or jimbals. Horizontal rays A, are directed down the tube by a plane mirror B, and are again rendered horizontal, and turned to the eye through a fide hole in the tube, by another mirror C. At the lower end is a pencil E fliding in a well-fitted focket, and prefled gently downwards by a weight or fpring; or ftill better, by the hand only. The refult or ufe is, that while the images are in fucceffion brought into apparent contact with a point in the field of view, the pencil may be employed in tracing them in true perfpective upon the table beneath *.

* There is an omiffion of the grey or rough glafs, if the drawing be meant for a camera; or of the eye-piece, if it be a telefcope. The firft focal convergence muft be made in thefe, and not at the eye.—N.

5   *Fig.*

Fig. 2 reprefents a fimple rule and ftring for drawing ovals Inftrument for on paper. A C B is a filken thread, fixed at A, and capable drawing ovals. of being lengthened, fhortened, and fixed by a fcrew B at the other end. This fcrew B can be placed, by a longitudinal groove in the ruler, at any diftance from A, and can be made to pinch the thread upon any one of the divifions of the rule. At C is a pencil to be moved in the bend of the thread. It muft be held upright, and it would be eafy to contrive means of keeping it fo; but it does not feem an object of fufficient neceffity to add to the price of the inftrument.

In the ufe, fet A at one focus of the intended oval and B at the other. Allow the ftring to extend till the pencil marks the extremity of the conjugate diameter. Draw the femi-oval by moving the pencil along in the ftretched thread: Then reverfe the points A and B, placing them refpectively on the foci occupied before by each other. Draw the other femi-oval, which completes the figure.

" I am, Sir,

Your obliged correfpondent,

R. B.

XIV.

On the Computation of Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR,   *Sept.* 8, 1804.

AS I can employ my time in a more ufeful, pleafant, and Introduction. advantageous way than by making tables of any kind, I have no defire on my own account to trouble your readers with the following remarks, and fhall therefore leave it entirely to your choice to notice them or not in your Journal.

I am, Sir,

Your humble fervant,

H. G.

YOUR correfpondent E. O. in his note, p. 79 of your On the compu- Journal for this month, has very properly pointed out an error tation of tables of fquares and in cubes.

On the Compu-
tation of tables
of fquares and
cubes.

in the rule laid down by me in p. 150 of your Journal for July laft, which will be corrected by fubftituting the word *given* for the word *next*.

But he does not feem to be aware that the rule I propofed for finding the fquares of roots in an arithmetical progreffion of thofe roots, is precifely the fame as that he has ufed and elucidated in pages 5 and 6 of your Journal for this month.

Neither has he made me a convert to his doctrine, that addition fhould be ufed in preference to fubtraction; for the one appears to me as eafy an operation as the other, for all the purpofes to which it is applied in the conftruction of the tables in queftion.

With refpect to the conftruction of the table of cubes, I will barely ftate my method of computing the cubes of the fame roots E. O. has calculated, in pages 8 and 9 of your Journal for this month, and will leave it to thofe who may be inclinable to enter on the inveftigation, to determine which method will be moft advantageoufly practifed, his or mine, only obferving, that E. O. has a continual repetition of the firft differences, which I have entirely avoided without adding any figures, in other refpects, to thofe he has ufed.

I ftill think my method of computing the table of cubes is not more liable to inaccuracy than that E. O. has adopted; but in order to avoid almoft the poffibility of error in my computation, and at the fame time to add to, examine, and correct Mr. Councer's table, I would place the cubes of half the given roots × 8. at fuch diftances under each other, as would enable me to interpolate my calculations of the cubes of twice thofe roots, as it is done in the left hand part of the following elucidation; and then it is evident, every other cube obtained by the calculation muft be the fame as that obtained from the table; and thus a proof of the accuracy of Mr. Councer's table, and of the continuation of it, would go hand in hand.

H. G.'s Method to obtain the Cubes of certain Roots.

Cubes.	Roots.
a = 26560 =	18736316416000 = A
b = 26559 =	18734200194879 = B
	2116221121 = C = A − B
	159360 = D = 6a
c = 26561 =	18738432796481 = E = D+C+A
	2116380481 = F = E − A
	159366 = G =
d = 26562 =	18740549336328 = H = G+F+E
	2116539847 = I = H − E
	159372 = K = G+6 = 6d
e = 26563 =	18742666035547 = L = K+I+H
	2116699219 = M = L − H
	159378 = N = K+6 = 6e
f = 26564 =	18744782894144 = O = N+M+L
	2116858597 = P = O − L
	159384 = Q = N+6 = 6f
g = 26565 =	18746899912125 = R = Q+P+O

TABLE,
Suppofed Mr. Councer's.

Roots.	Cubes.	Cubes of H.-G.'s Roots per contra, or Cubes of twice the Roots opposite.
13280³ =	2342039552000 × 8 =	18736316416000
13281³ =	2342568667041 × 8 =	18740549336328
13282³ =	2343097861768 × 8 =	18744782894144

XV.

Letter to the Editor from Mr. WILLIAM HENRY, *in reply to* Mr. Gough.

Manchefler, September 13, 1804.

SIR,

Previous remarks.

NOTHING was farther from my intention, when I communicated to you the " Illuftrations of Mr. Dalton's Theory of Mixed Gafes," than to enter into controverfy refpecting a doctrine, to the defence of which I may naturally be fuppofed to be much lefs competent than its author. Yet it is certainly required of me, both by the refpectful attention due to your correfpondent Mr. Gough, and to others of your readers, either to explain and fupport, or to relinquifh, if erroneous, the opinions refpecting which I have publicly expreffed a coincidence with Mr. Dalton. The laft alternative I do not, at prefent, feel difpofed to adopt, becaufe I am far from being convinced by Mr. G.'s reafoning; and in the explanation, which I am about to offer, I fhall confine myfelf to thofe proofs of the new theory of mixed gafes, which are furnifhed by my own experiments; leaving to Mr. Dalton the more important office of eftablifhing its fundamental evidences.

The quantity of gas abforbed being as the preffure, the laft is taken to be the caufe,

It is by no means clear to me, whether or not Mr. Gough denies the principle, " that the relation between gafes and water is altogether a mechanical one."* To me this appears as legitimate an inference, as can poffibly be deduced in phyfics; for the quantity of every gas, abforbed by water, follows exactly the ratio of the preffure: And, fince it is a rule in philofophizing, that effects of the fame kind, though differing in degree, are produced by the fame caufe, it is perfectly fafe to conclude, that every, even the minuteft portion of any gas, in a ftate of abforption by water, is retained entirely by incumbent preffure. There is no occafion, therefore, to call in the aid of the law of chemical affinity, when a mechanical law fully and fatisfactorily explains the appearances. And when the effect ceafes, it is equally conformable to juft reafoning to infer, that this happens, in every cafe, folely in confequence

* Certain acid gafes, the muriatic for inftance, are obvioufly excluded.

of

of the relaxation or removal of that mechanical power, which held the gas in its fituation. Under all circumftances, there-fore, when a gas efcapes from water, whether by placing the aqueous mixture under an exhaufted receiver, or in an atmof-phere of a different gas, the caufe operating its efcape, muft be one and identical, viz. the diminution of mechanical pref-fure. Before we account for any effect by comparifon of af-finities, the affinities themfelves fhould be proved to exift. But with refpect to the relation of gafes to water, the proof fails in every inftance; for how can that effect be fairly afcribed to chemical affinity which is deftroyed, as is the connection of every gas with water, by an unmixed mechanical caufe; and is it not abfurd to compare powers which have no exiftence in nature?

Admitting then the connection between gafes and water to be entirely dependent on phyfical preffure, there naturally arifes out of this law, an explanation of the curious fact, which I have afcertained, that each gas, when abforbed by water, is retained in its place by an atmofphere of no other gas but of its own kind. Under any other atmofphere, the abforbed gas efcapes, even without agitation, though this certainly acce-lerates the event. Now the fubaqueous gas can only accom-plifh its change of place by virtue of fome active principle or power inherent in it, and caufing its movement; and this power is its elafticity, which is not counteracted by that of an incumbent gas of a different fort. The fact affords, therefore, fomething further than " probability," that the particles of gafes prefs only on thofe of their own kind; for to fay that the elafticity of the fubaqueous gas is not counteracted by any in-cumbent one of a different fort, is to affert in other terms, that the one is not preffed by the other,—the principle which I am folicitous to eftablifh.

—and the pref-fure is of no other gas than that abforbed.

The above remarks are the only ones which I deem it within my province to urge in reply to Mr. Gough. Before clofing this letter, however, I muft exculpate myfelf from the charge of attempting to uphold a mechanical theory, by probabilities drawn from chemical facts. This ftatement is not correct; the facts which I have alledged are purely ftatical, and my object has been to prove that they were before erroneoufly included under the laws of chemical affinity, with which, in reality they have no connection. Thefe mechanical phenomena, I have

The theory is an induction from facts; capable of mathematical treatment, but founded on ob-fervation.

have brought in fupport of the general principle " that the particles of gafes prefs only on thofe of their own kind;" and in fo applying them, I truft I have conformed to the rigorous laws of philofophical induction. Such general principles are not uncommon in natural fcience; and though, like the law of gravitation and fome other lefs comprehenfive ones, they may be mathematically purfued and inveftigated, yet they are not derived from mathematical reafoning, but from a method of enquiry which Newton himfelf did not difdain to employ,—— that of induction on the bafis of experiment and obfervation. The fpeculations of Mr. Dalton, being founded on well afcertained facts, appear therefore to me to be fairly entitled to the appellation of *theory*, and not to be included within the definition of *hypothefis*,* handed down to us from the father of experimental philofophy. I remain, very truly, Sir,

Your's, &c.

WILLIAM HENRY.

XVI.

Defcription of a very fimple and cheap Contrivance for making Port Folios of large Dimenfions. By the late JAMES MALTON, *Efq.*†

SIR,

Method of conftructing large port-folios.

AS I well know the great inconvenience experienced by artifts and collectors of prints and drawings, from the want of portfolios of dimenfions capable of inclofing large fubjects, and as I alfo well know that the means ufed by the Society for the Encouragement of Arts, &c. to promulgate knowledge and ufeful information are earneft as they are extenfive, I am induced to lay before that body a port-folio of my conftruction, which I perfuade myfelf poffeffes every advantage that can be wifhed.

The difficulty, or rather the impoffibility of obtaining cafes or port-folios, as large as are fometimes requifite, has given rife to many expenfive contrivances, to the fame end; or large

* " Quicquid ex phaenomenis non deducitur, *hypothefis vocanda eft.*" Princ. LIII. in Bruckeri Hift. Crit. Phil. Tom. IV. p. 646.

† Society of Arts, 1803,

prints,

prints, &c. muſt be kept in rolls, to their almoſt certain de- ſtruction, by frequency of rolling; or at leaſt they are thus expoſed to the danger of being cruſhed by accident. Milled paſteboards, of which port-folios are made, are not manufactured above a certain moderate ſize: to exceed that ſize in a port-folio, is an undertaking of no inconſiderable trouble, in paſting, glueing, and preſſing them together. On inquiring of Mr. Newman, of Soho-ſquare, (a manufacturer of theſe articles) how he managed to make port-folios above the ordinary dimenſions, he informed me, it was an undertaking of trouble, and related his having made one for a gentleman, by attaching ſixteen of the largeſt milled boards together; that the materials alone coſt five guineas; and that its weight was greater than one man could lift.

My method of conſtruction obviates all diſadvantages— weight, expence, and trouble; and port folios of any dimenſions may very readily be manufactured by the ſimple application of two ſtraining-frames, covered on both ſides with canvas, and papered; and connected, as all port-folios are, by leather at the back, or with wooden backs, the ſides being connected by hinges. Thus a port-folio may be made capable of holding the largeſt cartoons, maps, and prints; and poſſeſſing another great advantage, beſides that of not bellying or ſwagging, when laid againſt a wall, as thoſe conſtructed of paſteboard do, to their own deſtruction, and material injury of the things they contain.

A frame of four feet by three will be ſtrong enough, if made of deal. The ſtiles are four inches wide by half an inch thick; and they have a middle upright ſtile of the ſame width, with angle pieces at the corners, as is ſhown in the engraving, *Fig.* 1. A frame of much greater dimenſions may require two middle upright ſtiles; and, if very large, a middle longitudinal ſtile, as is ſhown in the engraving, *Fig.* 2.

If the frames are made of mahogany, they need not exceed 3-16ths of an inch in thickneſs; but, of whatever wood they are made, it muſt be well ſeaſoned, or they will warp. A padlock may be applied to ſuch caſes, for the protection of their contents.

On this conſtruction I have made two port-folios, one of which I have had in uſe theſe ten years. A handſome one, of tolerably large dimenſions, I have ſent with this paper, for

the infpection of the Society. The outer ftiles of it are of mahogany, which, beaded, forms the out edge in a neat manner. Its fimple formation, its lightnefs, and its firm flatnefs, muft be obvious to every one; and I am of opinion the Society will obtain the thanks of all collectors and artifts (if they think it worthy of infertion in the Volume of their Tranfactions) by making this fimple matter publicly known. For my own part, I fhall be highly gratified in having contributed to the comfort of artifts and collectors, in preferving their valuable refearches.

<div align="center">I am, Sir,

Your obedient Servant,

JAMES MALTON.</div>

Norton-ftreet, June 25, 1802.

<div align="center">*Reference to the Engraving, Plate* VIII.</div>

Fig. 4. One of the fides of the frame for a port-folio, the dimenfions four feet by three: it may be made of deal or fir wood. The ftiles, four inches wide by half an inch thick; the middle upright ftile to be of the fame width. It fhould have angle pieces within the corners, to keep them firm.

Fig. 3. Shows a fide of another frame, where much larger dimenfions are required; it fhould then have two upright ftiles, and a middle longitudinal ftile, all within the frame, and angle pieces at the corners. If it is made of mahogany, inftead of deal, the ftiles may be reduced nearly one-fourth in breadth and thicknefs; the wood, in either cafe, fhould be well feafoned, that it may not be liable to warp.

<div align="center">XVII.</div>

Experiments and Calculations relative to Phyfical Optics, By THOMAS YOUNG, *M. D. F. R. S. From the Philofophical Tranfactions for* 1804.

<div align="center">(Concluded from p. 64.)</div>

Exper. 2. THE crefted fringes defcribed by the ingenious and accurate GRIMALDI, afford an elegant variation of the preceding experiment, and an interefting example of a calculation grounded on it. When a fhadow is formed by an object which has a rectangular termination, befides the ufual external

ternal fringes, there are two or three alternations of colours, beginning from the line which bifects the angle, difpofed on each fide of it, in curves, which are convex towards the bifecting line, and which converge in fome degree towards it, as they become more remote from the angular point. Thefe fringes are alfo the joint effect of the light which is inflected directly towards the fhadow, from each of the two outlines of the object. For, if a fcreen be placed within a few inches of the object, fo as to receive only one of the edges of the fhadow, the whole of the fringes difappear. If, on the contrary, the rectangular point of the fcreen be oppofed to the point of the fhadow, fo as barely to receive the angle of the fhadow on its extremity, the fringes will remain undifturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

If we now proceed to examine the dimenfions of the fringes, under different circumftances, we may calculate the differences of the lengths of the paths defcribed by the portions of light, which have thus been proved to be concerned in producing thofe fringes ; and we fhall find, that where the lengths are equal, the light always remains white; but that, where either the brighteft light, or the light of any given colour, difappears and reappears, a firft, a fecond, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progreffion, as nearly as we can expect experiments of this kind to agree with each other. I fhall compare, in this point of view, the meafures deduced from feveral experiments of Newton, and from fome of my own.

Comparifon of meafures.

In the eighth and ninth obfervations of the third book of Newton's Optics, fome experiments are related, which, together with the third obfervation, will furnifh us with the data neceffary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the fun's light, admitted through a fmall aperture ; and the point of concourfe of the two firft dark lines bordering the fhadows of the refpective knives, was obferved at various diftances. The refults of fix obfervations are expreffed in the firft three lines of the firft Table. On the fuppofition that the dark line is produced by the firft interference of the light reflected from the edges of the knives, with the light paffing in a ftraight line between them,

Comparison of measures.

we may affign, by calculating the difference of the two paths, the interval for the firft difappearance of the brighteft light, as it is expreffed in the fourth line. The fecond Table contains the refults of a fimilar calculation, from Newton's obfervations on the fhadow of a hair ; and the third, from fome experiments of my own, of the fame nature : the fecond bright line being fuppofed to correfpond to a double interval, the fecond dark line to a triple interval, and the fucceeding lines to depend on a continuation of the progreffion: The unit of all the Tables is an inch.

TABLE I. *Obf.* 9. N.

Diftance of the knives from the aperture			-	-	-	101.

Diftances of the paper from the knives	$1\frac{1}{2}$,	$3\frac{1}{5}$,	$8\frac{3}{5}$,	32,	96,	131.
Diftances between the edges of the knives, oppofite to the point of concourfe -	.012,	.020,	.034,	.057,	.081,	087.

Interval of difappearance ,0000122, ,0000155, .0000182, ,0000167, ,0000166, .0000166.

TABLE II. *Obf.* 3. N.

Breadth of the hair	-	-	-	-	-	$\frac{1}{280}$.
Diftance of the hair from the aperture			-	-	-	144.
Diftances of the fcale from the aperture	-		-		150,	252.
(Breadths of the fhadow	-	-	-		$\frac{1}{34}$,	$\frac{1}{5}$.)
Breadth between the fecond pair of bright lines					$\frac{2}{47}$,	$\frac{4}{77}$.
Interval of difappearance, or half the difference of the paths - - - - -					.0000151,	.0000173.
Breadth between the third pair of bright lines					$\frac{4}{77}$,	$\frac{3}{10}$.
Interval of difappearance, $\frac{1}{4}$ of the difference					.0000130,	.0000143.

TABLE III. *Exper.* 3.

Breadth of the object	.	-	-	-	-	.434,
Diftance of the object from the aperture			-	-	-	125.
Diftance of the wall from the aperture		-	-	-	-	250.
Diftance of the fecond pair of dark lines from each other						1..167.
Interval of difappearance, $\frac{1}{3}$ of the difference				-	-	.0000149.

Exper.

Exper. 4.

Breadth of the wire - , - - - .083.

Diftance of the wire from the aperture - - - 32.

Diftance of the wall from the aperture - - - 250.

(Breadth of the fhadow by three
measurements - - .815, .826, or .827; mean, .823.)

Diftance of the firft pair of dark
lines - - - - 1.165, 1.170, or 1.160; mean, 1.165.

Interval of difappearance - - - - .0000194.

Diftance of the fecond pair of dark
lines - - - - 1.402, 1.395, or 1.400; mean, 1.399.

Interval of difappearance - - - - .0000137.

Diftance of the third pair of dark
lines - - - - 1.594, 1.580, or 1.585; mean, 1.586.

Interval of difappearance - - - - .0000128.

It appears, from five of the fix obfervations of the firft Table, in which the diftance of the fhadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increafed in the ratio of 7 to 1, that the difference of the routes confti- tuting the interval of difappearance, varied but one-eleventh at moft; and that, in three out of the five, it agreed with the mean, cither exactly, or within $\frac{1}{160}$ part. Hence we are war- ranted in inferring, that the interval appropriate to the extinction of the brighteft light, is cither accurately or very nearly conftant.

But it may be inferred, from a comparifon of all the other obfervations, that when the obliquity of the reflection is very great, fome circumftance takes place, which caufes the interval thus calculated to be fomewhat greater : thus, in the eleventh line of the third Table, it comes out one-fixth greater than the mean of the five already mentioned. On the other hand, the mean of two of Newton's experiments and one of mine, is a refult about one-fourth lefs than the former. With refpect to the nature of this circumftance, I cannot at prefent form a de- cided opinion; but I conjecture that it is a deviation of fome of the light concerned, from the rectilinear direction affigned to it, arifing cither from its natural diffraction, by which the mag- nitude of the fhadow is alfo enlarged, or from fome other un- known caufe. If we imagined the fhadow of the wire, and the

5 fringes

fringes neareft it, to be fo contracted that the motion of the light bounding the fhadow might be rectilinear, we fhould thus make a fufficient compenfation for this deviation; but it is difficult to point out what precife tract of the light would caufe it to require this correction.

The mean of the three experiments which appear to have been leaft affected by this unknown deviation, gives .0000127 for the interval appropriate to the difappearance of the brighteft light; and it may be inferred, that if they had been wholly exempted from its effects, the meafure would have been fomewhat fmaller. Now the analogous interval, deduced from the experiments of Newton on thin plates, is .0000112, which is about one-eighth lefs than the former refult; and this appears to be a coincidence fully fufficient to authorife us to attribute thefe two claffes of phenomena to the fame caufe. It is very eafily fhown, with refpect to the colours of thin plates, that each kind of light difappears and reappears, where the differences of the routes of two of its portions are in arithmetical progreffion; and we have feen, that the fame law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The diftribution of the colours is alfo fo fimilar in both cafes, as to point immediately to a fimilarity in the caufes. In the thirteenth obfervation of the fecond part of the firft book, Newton relates, that the interval of the glaffes where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh obfervation of the third book, that the diftances between the fringes, under the fame circumftances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the fquares, in order to find the relation of the difference of the routes, we have the proportion of 14 to 9¼, which fcarcely differs from the proportion obferved in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crefted fringes of Grimaldi, already defcribed; for it will appear that, under the circumftances of the experiment related, the points in which the differences of the lengths of the paths defcribed by the two portions of light are equal to a conftant quantity, and in which, therefore, the fame kinds of light ought to appear or difappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the fhadow,

fhadow, and the afymptotes nearly with the diagonal line.
Such, therefore, muft be the direction of the fringes; and
this conclufion agrees perfectly with the obfervation. But it
muft be remarked, that the parts near the outlines of the fha-
dow, are fo much fhaded off, as to render the character of the
curve fomewhat lefs decidedly marked where it approaches to
its axis. Thefe fringes have a flight refemblance to the hy-
perbolic fringes obferved by Newton; but the analogy is only
diftant.

III. APPLICATION TO THE SUPERNUMERARY RAINBOWS.

The repetitions of colours fometimes obferved within the
common rainbow, and defcribed in the Philofophical Tranfac-
tions, by Dr. Langwith and Mr Daval, admit alfo a very eafy
and complete explanation from the fame principles. Dr. Pem-
berton has attempted to point out an analogy between thefe co-
lours and thofe of thin plates; but the irregular reflection
from the pofterior furface of the drop, to which alone he attri-
butes the appearance, muft be far too weak to produce any
vifible effects. In order to underftand the phenomenon, we have
only to attend to the two portions of light which are exhibited
in the common diagrams explanatory of the rainbow, regularly
reflected from the pofterior furface of the drop, and crofling
each other in various directions, till, at the angle of the great-
eft deviation, they coincide with each other, fo as to produce,
by the greater intenfity of this redoubled light, the common
rainbow of 41 degrees. Other parts of thefe two portions
will quit the drop in directions parallel to each other; and thefe
would exhibit a continued diffufion of fainter light, for 25°
within the bright termination which forms the rainbow, but
for the general law of interference, which, as in other fimilar
cafes, divides the light into concentric rings; the magnitude
of thefe rings depending on that of the drop, according to the
difference of time occupied in the paffage of the two portions,
which thus proceed in parallel directions to the fpectator's eye,
after having been differently refracted and reflected within the
drop. This difference varies at firft, nearly as the fquare of
the angular diftance from the primitive rainbow: and, if the
firft additional red be at the diftance of 2° from the red of the
rainbow, fo as to interfere a little with the primitive violet,
the fourth additional red will be at a diftance of nearly 2°
more;

more; and the intermediate colours will occupy a fpace nearly equal to the original rainbow. In order to produce this effect, the drops muft be about $\frac{1}{76}$ of an inch, or .013, in diameter: it would be fufficient if they were between $\frac{1}{76}$ and $\frac{1}{80}$. The reafon that fuch fupernumerary colours are not often feen, muft be, that it does not often happen that drops fo nearly equal are found together: but, that this may fometimes happen, is not in itfelf at all improbable: we meafure even medicines by dropping them from a phial, and it may eafily be conceived that the drops formed by natural operations may fometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the obfervation, may beft be determined from Dr. Langwith's own words.

" Auguft the 21ft, 1722, about half an hour paft five in the evening, weather temperate, wind at north-eaft, the appearrance was as follows: The colours of the primary rainbow were as ufual, only the purple very much inclining to red, and well defined: under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more dufky green: under this were alternately two arches of reddifh purple, and two of green: under all, a faint appearance of another arch of purple, which vanifhed and returned feveral times fo quick, that we could not readily fix our eyes upon it. Thus the order of the colours was, 1. Red, orange-colour, yellow, green, light blue, deep blue, purple. 11. Light green, dark green, purple. 111. Green, purple. 1v. Green, faint vanifhing purple. You fee we had here four orders of colours, and perhaps the beginning of a fifth; for I make no queftion but that what I call the purple, is a mixture of the purple of each of the upper feries with the red of the next below it, and the green a mixture of the intermediate colours. I fend you not this account barely upon the credit of my own eyes; for there was a clergyman and four other gentlemen in company, whom I defired to view the colours attentively, who all agreed, that they appeared in the manner that I have now defcribed. There are two things which well deferve to be taken notice of, as they may perhaps direct us, in fome meafure, to the folution of this curious phenomenon. The firft is, that the breadth of the firft feries fo far exceeded that of any of the reft, that, as near as I could judge, it was equal to them all taken together. The fecond is, that I have never obferved thefe inner orders

of

of colours in the lower parts of the rainbow, though they have often been incomparably more vivid than the upper parts, under which the colours have appeared. I have taken notice of this fo very often, that I can hardly look upon it to be accidental ; and, if it fhould prove true in general, it will bring the difquifition into a narrow compafs; for it will fhow that this effect depends upon fome property which the drops retain, whilft they are in the upper part of the air, but lofe as they come lower, and are more mixed with one another." Phil. Tranf. Vol. XXXII. p. 243.

From a confideration of the nature of the fecondary rainbow, of 54°, it may be inferred, that if any fuch fupernumerary colours, were feen attending this rainbow, they would necelfarily be external to it, inftead of internal. The circles fometimes feen encompaffing the obferver's fhadow in a mift, are perhaps more nearly related to the common colours of thin plates as feen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

The experiment of Grimaldi, on the crefted fringes within the fhadow, together with feveral others of his obfervations, equally important, has been left unnoticed by Newton. Thofe who are attached to the Newtonian theory of light, or to the hypothefes of modern opticians, founded on views ftill lefs enlarged, would do well to endeavour to imagine any thing like an explanation of thefe experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at leaft from idle declamation againft a fyftem which is founded on the accuracy of its application to all thefe facts; and to a thoufand others of a fimilar nature.

Argumentative inferences.

From the experiments and calculations which have been premifed, we may be allowed to infer, that homogeneous light, at certain equal diftances in the direction of is motion, is poffeffed of oppofite qualities, capable of neutralifing or deftroying each other, and of extinguifhing the light, where they happen to be united ; that thefe qualities fucceed each other alternately in fucceffive concentric fuperficies, at diftances which are conftant for the fame light, paffing through the fame medium. From the agreement of the meafures, and from the fimilarity of the phenomena, we may conclude,

Homogeneous light has oppofite qualities at equal diftances along its courfe, by which neutralization or extinction of the light may be effected.

that

that thefe intervals are the fame as are concerned in the production of the colours of thin plates; but thefe are fhown, by the experiments of Newton, to be the fmaller, the denfer the medium; and, fince it may be prefumed that their number muft neceffarily remain unaltered in a given quantity of light, it follows of courfe, that light moves more flowly in a denfer, than in a rarer medium: and this being granted, it muft be allowed, that refraction is not the effect of an attractive force directed to a denfer medium. The advocates for the projectile hypothefis of light, muft confider which link in this chain of reafoning they may judge to be the moft feeble; for, hitherto, I have advanced in this Paper no general hypothefis whatever. But, fince we know that found diverges in concentric fuperficies, and that mufical founds confift of oppofite qualities, capable of neutralifing each other, and fucceeding at certain equal intervals, which are different according to the difference of the note, we are fully authorifed to conclude, that there muft be fome ftrong refemblance between the nature of found and that of light.

Light moves more flowly in denfer mediums.

Light and found ftrongly refemble each other.

I have not, in the courfe of thefe inveftigations, found any reafon to fuppofe the prefence of fuch an inflecting medium in the neighbourhood of denfe fubftances as I was formerly inclined to attribute to them; and, upon confidering the phenomena of the aberration of the ftars, I am difpofed to believe, that the luminiferous ether pervades the fubftance of all material bodies with little or no refiftance, as freely perhaps as the wind paffes through a grove of trees.

There is probably no inflecting medium.

The obfervations on the effects of diffraction and interference, may perhaps fometimes be applied to a practical purpofe, in making us cautious in our conclufions refpecting the appearances of minute bodies viewed in a microfcope. The fhadow of a fibre, however opaque, placed in a pencil of light admitted through a fmall aperture, is always fomewhat lefs dark in the middle of its breadth than in the parts on each fide. A fimilar effect may alfo take place, in fome degree, with refpect to the image on the retina, and imprefs the fenfe with an idea of a tranfparency which has no real exiftence: and, if a fmall portion of light be really tranfmitted through the fubftance, this may again be deftroyed by its interference with the diffracted light, and produce an appearance of partial opacity, inftead of uniform femitranf-

Practical application of the facts.

parency.

parepcy. Thus, a central dark fpot, and a light fpot fur-rounded by a darker circle, may refpeÕively be produced in the images of a femitranfparent and an opaque corpufcle ; and imprefs us with an idea of a complication of flruÕure which does not exift. In order to deteÕ the fallary, we may make two or three fibres crofs each other, and view a number of globules contiguous to each other; or we may obtain a ftill more effeÕual remedy by changing the magni-fying power; and then, if the appearance remain conftant in kind and in degree, we may be affured that it truly repre-fents the nature of the fubftance to be examined. It is na-tural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewfon in the Phil. Tranf. Vol. LXIII. for 1773, might not in fome meafure have been in-fluenced by a deception of this kind : but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch focus, I have found them nearly fuch as Mr. Hewfon has defcribed them.

REMARKS ON THE COLOURS OF NATURAL BODIES.

Exper. 5. I have already adduced, in illuftration of New-ton's comparifon of the colours of natural bodies with thofe of thin plates, Dr. Wollafton's obfervations on the blue light of the lower part of a candle, which appears, when viewed through a prifm, to be divided into five portions. I have lately obferved a fimilar inftance, ftill more ftrongly marked, in the light tranfmitted by the blue glafs fold by the opti-cians. This light is feparated by the prifm into feven dif-tinÕ portions, nearly equal in magnitude, but fomewhat broader, and lefs accurately defined, towårds the violet end of the fpeÕrum. The firft two are red, the third is yellow-ifh green, the fourth green, the fifth blue, the fixth bluifh violet, and the feventh violet. This divifion agrees very nearly with that of the light refleÕed by a plate of air $\frac{1}{6840}$ of an inch in thicknefs, correfponding to the 11th feries of red and the 18th of violet. A fimilar plate of metallic oxide, would perhaps be about $\frac{1}{15600}$ of an inch in thicknefs. But it muft be confeffed, that there are ftrong reafons for believing the colouring particles of natural bodies in general to be in-comparably finaller than this; and it is probable that the analogy, fuggefted by Newton, is fomewhat lefs clofe than

he

he imagined. The light reflected by a plate of air, at any thicknefs nearly correfponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumftances, the 11th red and the neighbouring colours may ftill be diftinguifhed. The light of fome kinds of coloured glafs is pure red; that of others, red with a little green: fome intercept all the light, except the extreme red and the blue. In the blue light of a candle, expanded by the prifm, the portions of each colour appear to be narrower, and the intervening dark fpaces wider, than in the analogous fpectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye fold in the fhops, which is a preparation of the carthamus, affords a good fpecimen of a yellow green light regularly reflected, and a crimfon probably produced by tranfmiffion.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Dark rays of Ritter obferved alfo by Dr. Wollafton.

Exper. 6. The exiftence of folar rays accompanying light, more refrangible than the violet rays, and cognifable by their chemical effects, was firft afcertained by Mr. Ritter: but Dr. Wollafton made the fame experiments a very fhort time afterwards, without having been informed of what had been done on the Continent. Thefe rays appear to extend beyond the violet rays of the prifmatic fpectrum, through a fpace nearly equal to that which is occupied by the violet. In order to complete the comparifon of their properties with thofe of vifible light, I was

They are reflected from a thin plate of air with the fame modification of rings as vifible light is.

defirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpofe, I formed an image of the rings, by means of the folar microfcope, with the apparatus which I have defcribed in the Journals of the Royal Inftitution, and I threw this image on paper dipped in a folution of nitrate of filver, placed at the diftance of about nine inches from the microfcope. In the courfe of an hour, portions of three dark rings were very diftinctly vifible, much fmaller than the brighteft rings of the coloured image, and coinciding very nearly, in their dimenfions, with the rings of violet light that appeared upon the interpofition of violet glafs. I thought the dark rings were a little fmaller than the violet rings, but the difference was not fufficiently great to be accurately afcertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It

is

is the lefs furprifing that the difference fhould be fo fmall, as the dimenfions of the coloured rings do not by any means vary at the violet end of the fpectrum, fo rapidly as at the red end. For performing this experiment with very great accuracy, a helioftate would be neceffary, fince the motion of the fun caufes a flight change in the place of the image; and leather, impregnated with the muriate of filver, would indicate the effect with greater delicacy. The experiment, however, in its prefent ftate, is fufficient to complete the analogy of the invifible with the vifible rays, and to fhow that they are equally liable to the general law which is the principal fubject of this Paper. If we had thermometers fufficiently delicate, it is probable that we might acquire, by fimilar means, information ftill more interefting, with refpect to the rays of invifible heat difcovered by Dr. Herfchel; but at prefent there is great reafon to doubt of the practicability of fuch an experiment.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

National Inftitute.

THE clafs of mathematical and phyfical fciences of the French National Inftitute had a public fitting on the 6th Meffidor (June 25.) The fubjects appointed for prizes were the following:

National Inftitute prizes.

Mathematics. "It is required to give a theory of the perturbations of the planet Pallas, difcovered by M. Olbers."

Planet Pallas.

The prize will be a gold medal, weighing one kilogramme, (35½oz. avoirdupois.)

Natural Philofophy. The clafs had propofed for the fubject of a prize the following queftion. "To determine by experiment the different fources of carbon in vegetables." The concurrence is prorogued until the 1ft Germinal in the year 13 (March 22, 1805.)

Carbon in vegetables.

The concurrence for the prize upon the following queftion is alfo prolonged to the fame period. "To determine by anatomical and chemical obfervations and experiments, what are the phenomena of the torpid ftate to which certain animals, fuch as marmots, dormice, &c. are fubject during the winter, with refpect to the circulation of the blood, refpiration, and irritability; to inveftigate the caufes of this fleep, and why it is peculiar to thefe animals."

Torpid ftate of dormice, &c.

The

The value of thefe two prizes is doubled, and confifts in two kilogrammes of gold about 6800 francs each (£289.)

The clafs had propofed for the fecond time, the 15th Germinal of the year 10, as the fubject of a prize to be decreed in the public fitting of Meffidor, the following queftion.

Ferments. "What are the characters which diftinguifh, in vegetable and animal matters, thofe bodies which ferve as ferments from thofe in which they produce the ftate of fermentation."

The memoirs received not having anfwered the conditions of the program, and the clafs confidering that this queftion has been four years before the public, the fubject is now withdrawn.

Aftronomy. Aftronomy. Mr. Piazzi of Palermo has obtained the prize
Prize gained by founded by M. De Lalande, in favour of the perfon who fhall
Piazzi. have made the moft interefting obfervation, or written the moft ufeful memoir to the progrefs of aftronomy. Mr. Piazzi is the difcoverer of the planet Ceres.

Readings. The readings delivered during the fitting were as follows

1. Notice of the labours of the clafs during the year; the mathematical part by M. Delambre. 2. Notice of their labours; the phyfical part by M. Cuvier. 3. Account of a phyfico-mathematical theory of currents of water, by M. Prony. 4. A note refpecting the amelioration of fheep in the fouth of France, by M. Teffier. 5. General reflections on the productions of the vegetable kingdom in the Pyrenean mountains by M. Raymond. 6. Reflections on heat, by Count Rumford. 7. Extract of a memoir tending to illuftrate the œconomical hiftory of vegetables, cultivated or naturally growing in the Canaries, by M. Brouffonet.

Extract of a Letter from PROFESSOR BODE, *Aftronomer Royal,
to Mr. A. F. Thoelden.*

Dated Berlin, Sept. 18.

Place of new ON the firft of September Mr. Harding difcovered, at
planets. Lilienthal, a new moveable ftar; it appears to be of the eighth magnitude; its motion retrograde towards the South. Probably this may be another new planet in the orbit of Ceres and Pallas.

1ft Sept.

1ft Sept. 10ʰ.—Even. RA. 2° 24'   Decl. 0° 37'—N.

5	—	11	8'	-	1	51	51"	- 0	11	26" S.
8	—	8	11	-	1	29	28	- 0	47	19 -
10	—	8	15	-	1	12	55	- 1	11	55 -

The two laft obfervations were made by Dr. Olbers, at Bremen. I received this intelligence yefterday, but at prefent this fmall planet (there are now three) is not difcoverable, on account of the moon-fhine.

Extract of a Letter from Mr. G. B. Grenough.

* * * * There is an overfight in the hafty account I fent you of the Abbé Melograni's blow-pipe. No provifion is made for admitting the external air. This may be done by feveral means. It may be admitted through that part of the axis which is oppofite the nozel; but as this would require fome contrivance which would render it defective in fimpli- city, I fhould prefer the addition of a fmall valve opening in- wards at that part of each veffel which is immediately oppo- fite the neck that joins them. Whether this method was ufed in the actual inftrument I cannot, at this diftance of time, re- collect.

Correction of a detail in the ac- count of Melo- grani's blow- pipe.

Organic Remains of a former World. An Examination of the Mineralized Remains of the Vegetables and Animals of the An- tediluvian World, generally termed extraneous Foffils. By James Parkinson, *Hoxton. Vol. I. containing the Vege- table Kingdom. 4to. pp. 471. with 10 Plates (£2 2 0) Robfon, &c.*

AS it would be impracticable in the general announcement of a work of the magnitude and importance of the prefent to give the reader any well filled outline of the plan, I muft con- fine myfelf to ftate in few words, that the defcription and elu- cidation of organic foffil bodies, though in many points of view of the higheft intereft, has not yet conftituted the fub- ject of a general treatife; notwithftanding the various confi- derations refpecting the local, the individual, and the chemi- cal hiftory of thefe wonderful bodies have occupied the re- fearches of many able philofophers in detached memoirs; and that the compofition of fuch an hiftory demands a great ex- ertion of labour, in confulting and digefting what has been

Parkinfon's or- ganic remains.

done

done by others, an accurate, geological, and chemical view of the facts upon which fcientific conclufions or deductions may be grounded, and above all, an ardent curiofity, with adequate leifure and means to become acquainted with the fpecimens themfelves:—And, after this preface, I muft add, that fortunately for the advancement of this branch of knowledge, the author of the prefent work has amply vindicated his claim to thefe requifites.

The work is written in the form of letters, and enlivened by an adoption of the ftyle and fome of the incidents of a traveller. Much of ufeful fcience and clear explanations of the fubject are to be found in the pages of this volume. The abridgements from other authors are full, and the quotations always given with the volume and page, which I mention more readily, as a flovenly habit of loofe quotation, or not quoting at all, is but too common, even with our writers of merit. Nine plates of the fubjects are given, which do the higheft credit to the defigner and engraver, Springfguth, for their delicacy and beauty, and confiderable pains have been taken in colouring them. The frontifpiece, reprefenting the Ark upon Mount Ararat, defigned by Corbould, is happy and appropriate; though the artift has taken a pictorial licence to reprefent the fun and the rainbow nearly on the fame azimuth, for which neither opticians nor correct obfervers will be difpofed to excufe him.

The volume yet to be publifhed will contain the animal kingdom; a fubject perhaps ftill more attractive than the vegetable. In the remains of extinct vegetables, we appear to contemplate the phyfical economy of a world long ago obliterated; but in thofe of animals, we feem to behold fomething that gives a glimpfe into the moral relations of thofe beings of paft ages who exifted upon its furface.

Apparatus for Filtration
by Sir H. C. Englefield Bart. &c.
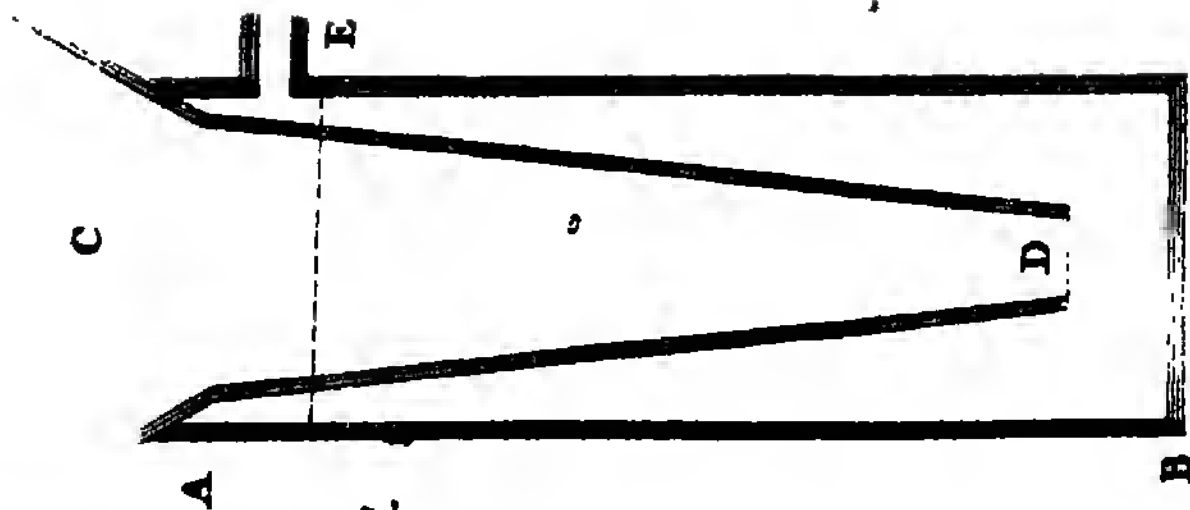
Apparatus for purifying a Stream
of Water.
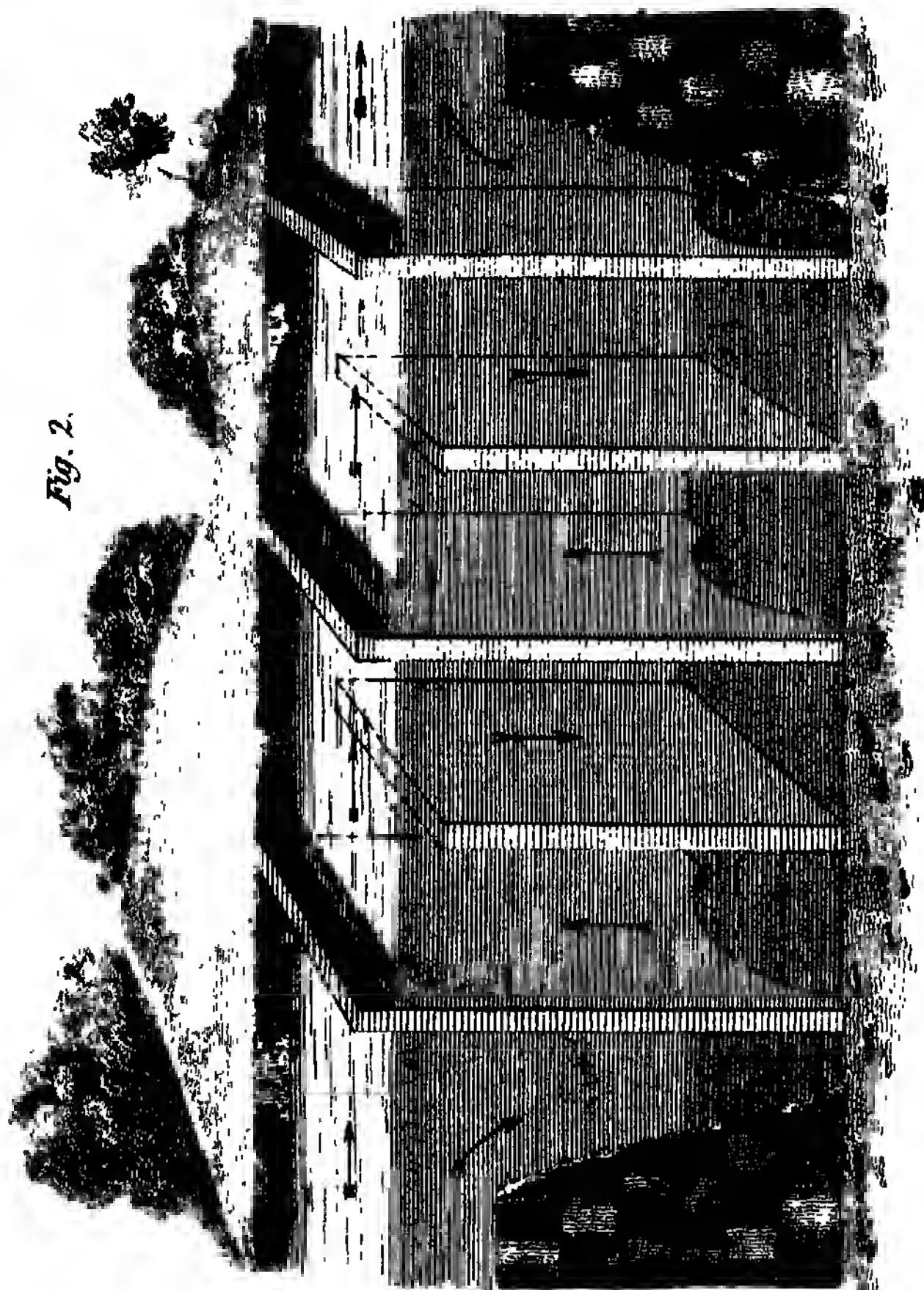
Fig. 1.
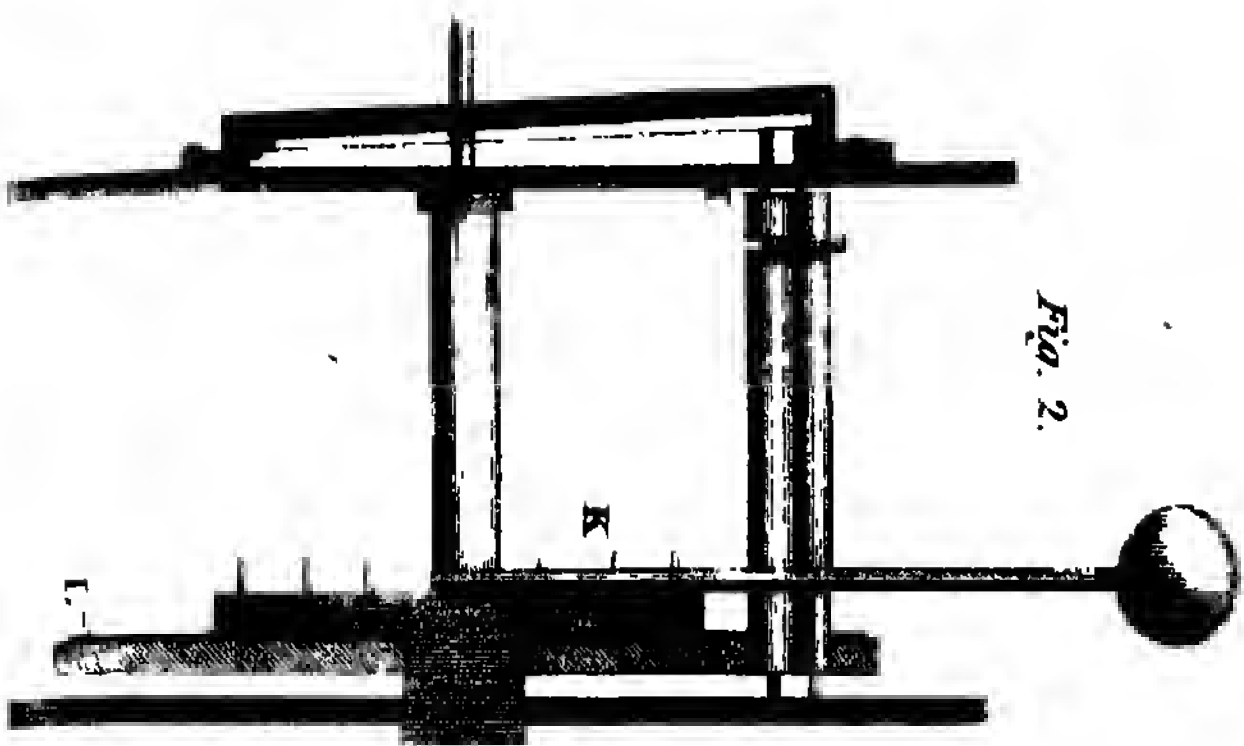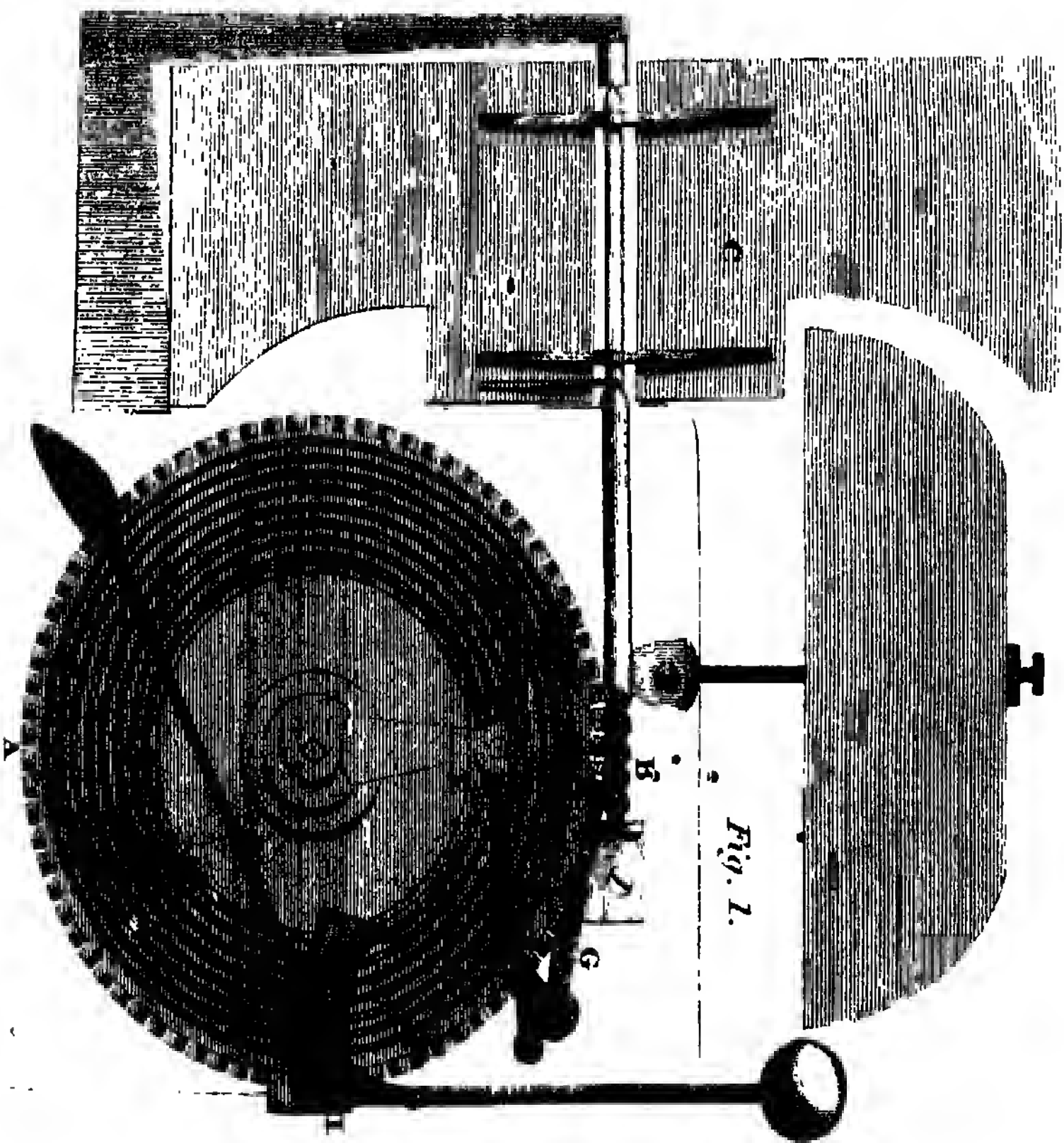
Fig. 2.

Fig. 2.

Fig. 1.

Mr. Antiss. Indicator of the Number of Draughts from a L. Mine.
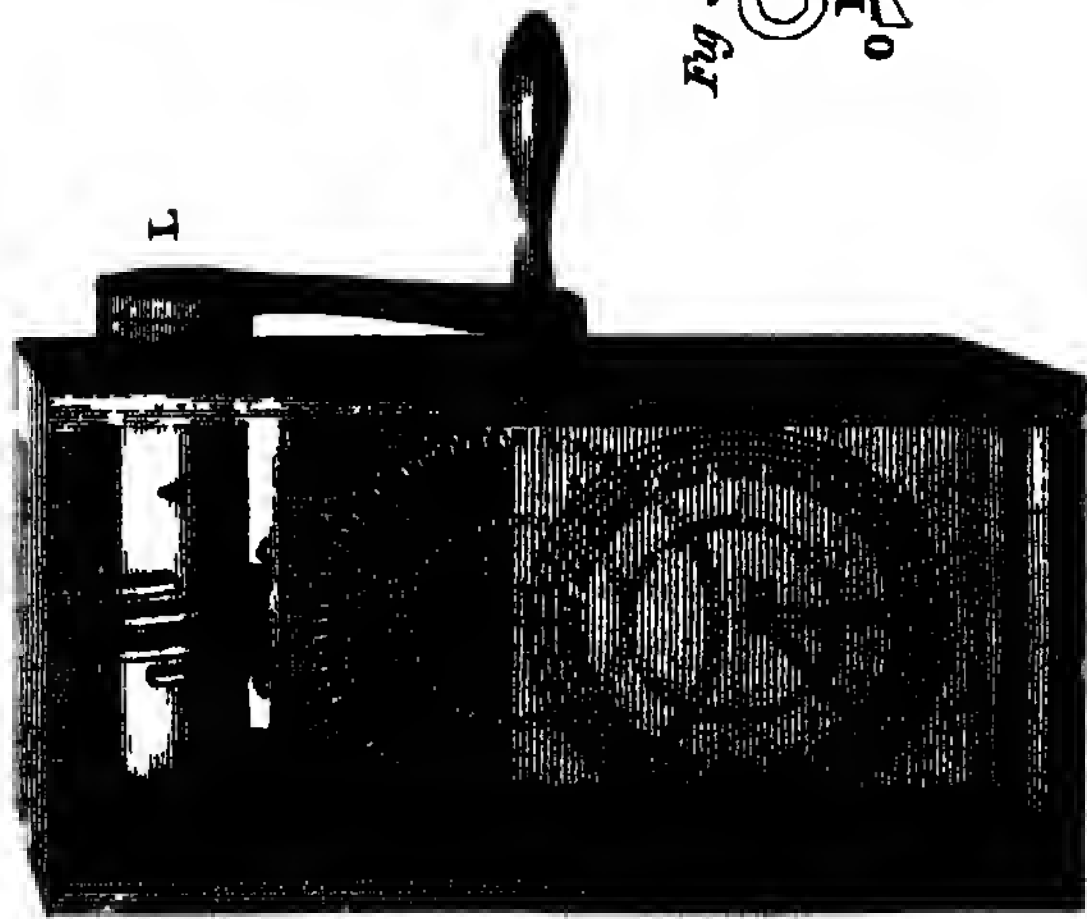
Fig. 1.

Fig. 2.
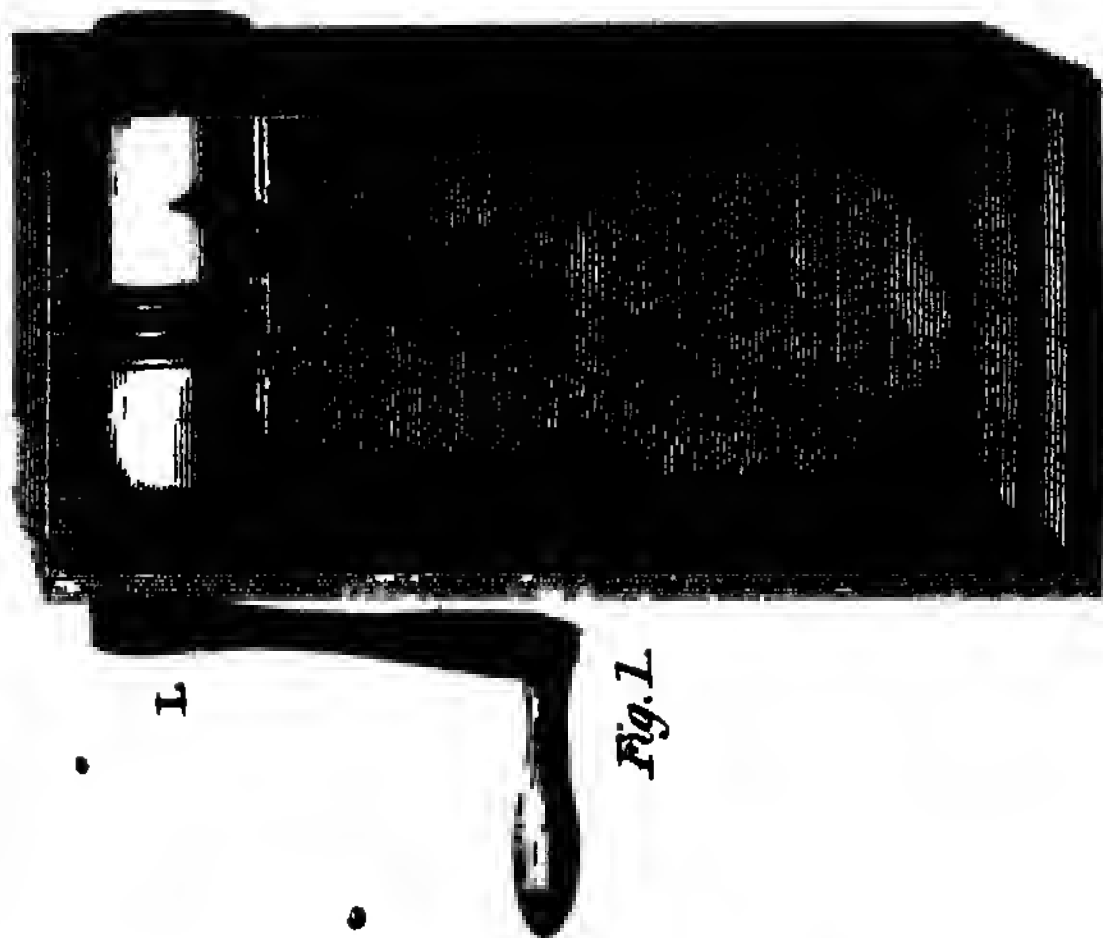
Fig. 3.

Perspective Instrument

Fig. 1
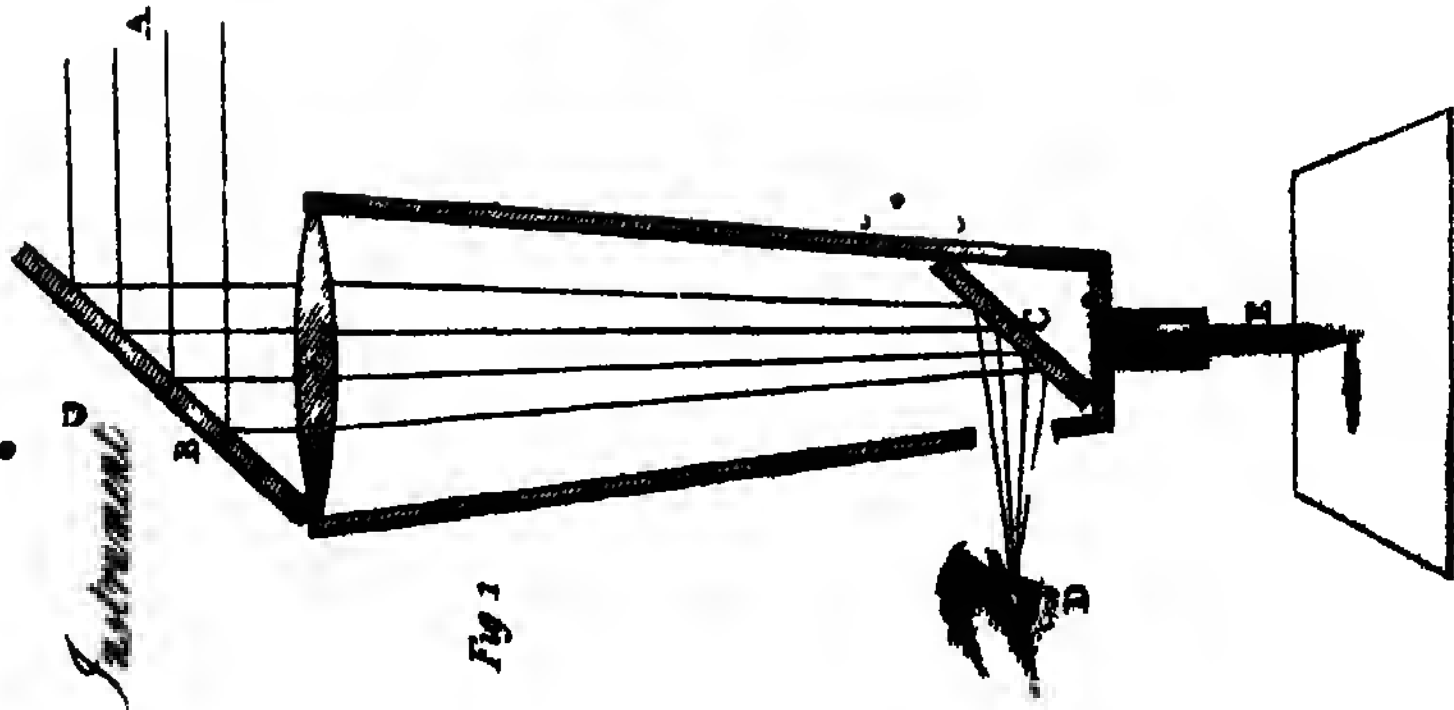
Instrument for drawing Ovals
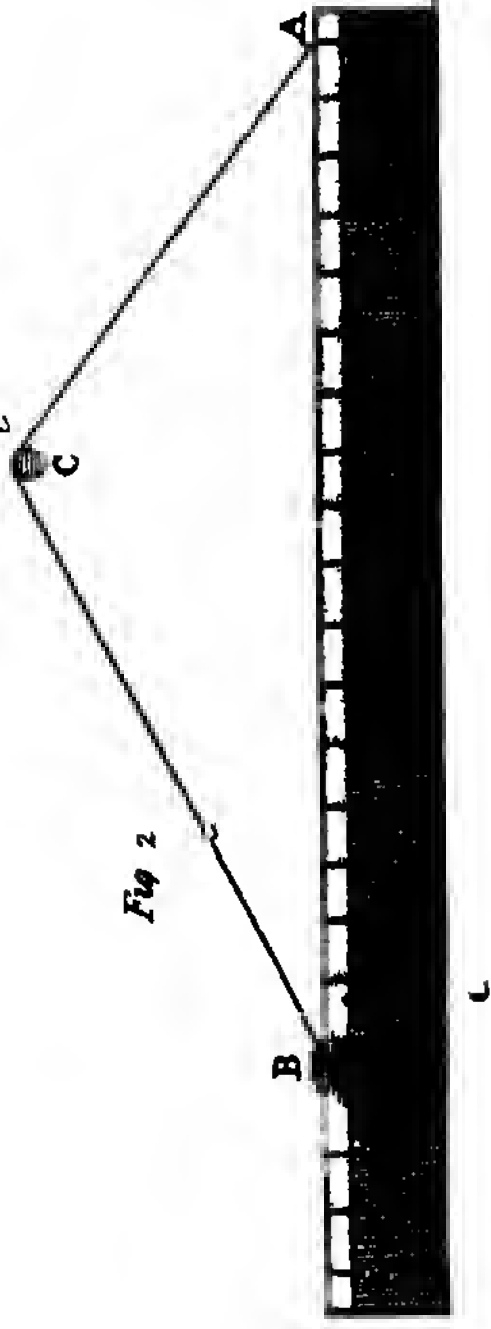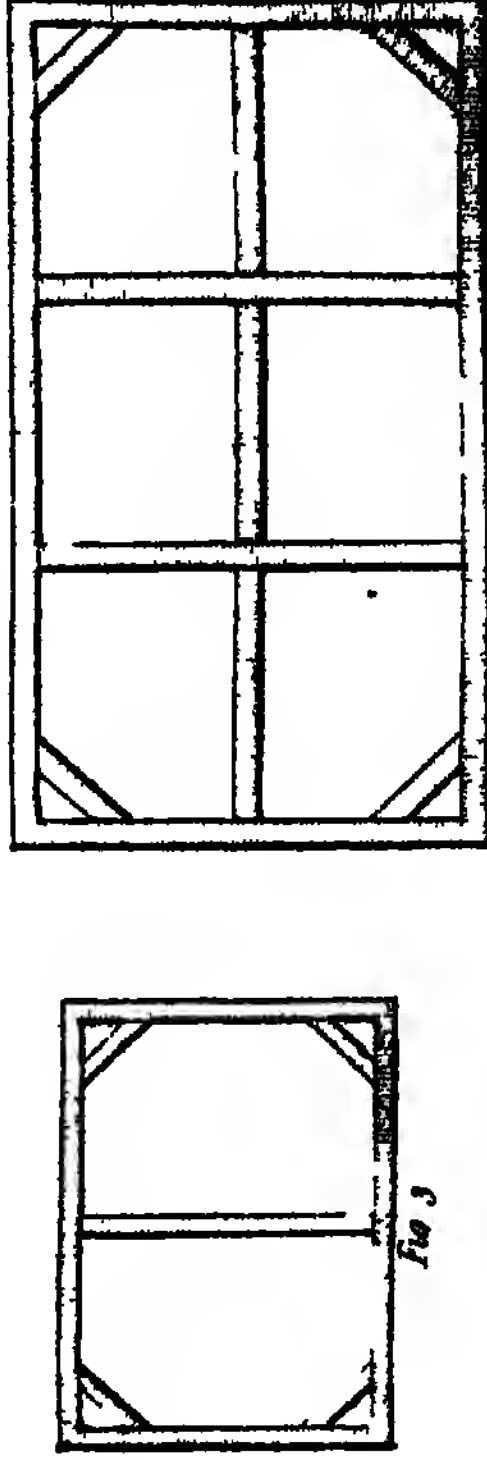
Fig. 2

Maltons Port-folios

Fig. 1

Fig. 3

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

•ARTICLE I.

Defcription of a Tallow Lamp, which regulates its Supply by a fpontaneous Movement. By Mr? JOHN WHITLEY BOSWELL. Communicated by the Inventor.

To Mr. NICHOLSON.

SIR,

THE conftant attention which candles demand from the frequent fnuffing they require, and the continued variation of their light in the intervals of this operation, on the one hand, and on the other the expence and trouble which oil lamps occafion, together with the difagreeable fcent they in general produce, have rendered the contrivance of a good lamp for burning tallow, a defirable object to feveral for a long time.

I hope, therefore, the defcription I fend of a lamp for this purpofe, which poffeffes fome valuable properties, will be acceptable to you.

The beft lamp for burning tallow which I had feen before I contrived this, was one invented by Mr. March, of Barn-ftaple, in which the tallow kept continually melted in a veffel fufpended over the flame, was admitted by a fmall pipe to the burner, and the fupply regulated by a kind of cock attached

Defcription of a lamp for burning tallow.

Defcription of a
lamp for burning
tallow.

to the pipe: But this method occafioned a wafte from the evaporation of the melted tallow, and befides, required much attendance to regulate the cock and keep the tallow veffel at a proper diftance from the flame; for the alteration of the temperature of the melted tallow, which was very frequent from many caufes, continually changing its degree of fluidity, caufed it to vary in its fupply through the pipe proportionally.

It occurred to me fome time after I faw this lamp, that one might be contrived in which the tallow fhould only be melted as it was confumed, and other of the inconveniences be removed to which the firft was fubject; and I made then a rough drawing of a lamp for this purpofe, but other more interefting matters prevented my having one made on that plan before laft December: It confifted of a trough fupported in an inclination of about 45 degrees, fimilar to the one in the figure, which held a fquare prifm of tallow; under this was placed a little oblong veffel of tin, formed fo as to move forwards and backwards like a drawer, in front of which was placed a burner: a couple of wires placed acrofs the lower part of the trough, prevented the tallow from fliding down beyond them, as it otherwife would from the obliquity of its fupport; but as it melted by the heat from the burner, it fell in drops into the little drawer, fo as to fupply the flame, while the degree of this fupply was regulated by moving the drawer in or out, fo as to bring the burner nearer to, or further from the tallow. In this ftate, though it formed a very ufeful lamp, it was not fo perfect as I could wifh; every alteration of temperature of the apartment (which in cold weather happens frequently) required a proportional alteration in the diftance of the burner from the tallow, to prevent the little drawer being either overflowed by a fuperabundance of melted tallow, or the flame ceafing from a deficiency of fupply. The attendance this circumftance required induced me to contrive the prefent lamp, or rather to improve the former, in manner following.

I conceived that if the little pan, which held the melted tallow and the burner, was placed on the arm of a balance inclined from 30 to 45 degrees from the perpendicular, and its weight compenfated by a counterpoife at the extremity of the other arm, that when more than a certain quantity of tallow

low ran into it, it wouid fink lower, and, in doing fo, move the burner farther forward from the trough at the fame time, and *rice verfa*, and thus regulate the fupply fpontaneoufly: But as it was neceffary that the pan (holding a fluid) fhould always maintain an horizontal pofition during its movement, I imagined this might be effe&ed by placing another arm beneath the balance-arm, and fixing a perpendicular piece, fo jointed to the extremity of each as. to move frefh and fteadily, which piece fhould fupport an horizontal ftand for the pan.

I had a lamp made in this manner, and found it anfwer my intention perfe&ly. It required no attendance whatfoever, but regulated its fupply with precifion; and afforded, likewife, an agreeable fpe&acle, having in its movements fomewhat the air of thofe of an animal, from their exa& relation to an evident obje&, and adopting themfelves to all its changes.

The lamp thus made affumed the form in the figure, Pl. IX. where T reprefents the inclined trough for holding the tallow; P the pan for catching the tallow,as it melted, and holding the burner; F the flame and burner; R the perpendicular piece fuftaining a fupport for the pan; B the balance moving freely. on its center in the ftand S, and jointed to R as before defcribed; *c* the parallel arm ufed to keep R perpendicular, and by its means horizontal; *w* the counter weight, placed fo as to move on the extremity of B by a fcrew. The firft frofty night fhewed me the neceffity of the addition of another part to remedy an inconvenience I had not forefeen; the dropping tallow affuming the form of an ificle, joined the pan to the trough, and thus ftopped its movement; *As* it was troublefome to break this off as it occurred, I contrived the following means to prevent its formation: A little oblong flidingpiece, the fame breadth as the trough, was placed clofe undefneath its lower part; the extremity of this flide was made angular, that the melted tallow might fall from a fingle point; a wire reprefented at L, moved it back or forwards as required: By this flide the drop may be made to fall as clofe to the ftream of burning air from the flame as is thought fit, and all congelation prevented. I afterwards added a fort of refle&or to the lamp, which confiderably improved the light, not merely a&ing as its name implies, but alfo occafioning an

increafe

Defcription of a
lamp for burning
tallow.
increafe in the current of air which paffed the flame,. ren_ dered it fomewhat more bright, and confiderably more fteady. This part is reprefented at D in the figure, and confifts of two fide-pieces placed before the trough, but fo as not to obftruct the tallow, and bent forward to form an angle with its fides. To improve the light, I alfo ufe in the lamp five fmall diftinct wicks, of three threads each, inftead of one large wick, placed near each other in a row, which renders the flame clear and free from fmoke, and thereby alfo prevents any unpleafant fcent. I ufed this lamp for more than three months, and found it very convenient for reading or writing by, as when once lighted it required no farther attention, and kept its light at nearly the fame height at all times, and of the fame degree of intenfity; and I can further recommend it, as yielding the greateft light at the fmalleft coft, in proportion, of any invention yet made public which is applicable to do- meftic ufes.

I have had two lamps of this kind carefully made by Mr. Lloyd, No. 178, Strand, and left with him as models: In one of which the chief object is cheapnefs, as far as confiftent with perfection and neatnefs; in the other, to form an elegant utenfil for the ftudy, office, or chamber; and any gentleman may be fupplied with them there, made exactly to my plan, on reafonable terms; and as Mr. Lloyd is the only perfon at prefent appointed to make them, and inftructed fully in the mechanifm, they can be had perfect from him only.

It is probable wax might alfo be burned in this lamp as well as tallow: of this I cannot fpeak from experience, but mention it, as I know fome would prefer to ufe it. .

I am, Sir,

Your very humble fervant,

J. WHITLEY BOSWELL..

Oct. 1, 1804.

II.

Account of a fuccefsful Cafe in which Deafnefs was cured by Puncture of the Membrana Tympani. By M. Maunoir, of Geneva. Communicated by ALEXANDER MARCET, M. D. Phyfician to Guy's Hofpital.

To Mr. NICHOLSON.

SIR,

THE inclofed communication on the effects produced in a cafe of deafnefs by the perforation of the tympanum of the ear, is extracted from a letter which I have juft received from Mr. Maunoir, an eminent furgeon at Geneva. The hiftory of this cafe, drawn up by fuch an intelligent and accurate obferver, appears to me curious, both from the phyfiological remarks which it contains, and from the ftriking illuftration which it affords of a new mode of practice lately propofed in this country. And, as this fubject feems fully as interefting in its philofophical point of view, as in its connection with practical furgery, I fhould wifh, if you think it proper, that you would give it a place in the next number of your valuable Journal. I have the honour to be, Sir,

<div style="text-align:right">

Your obedient humble fervant,

ALEXANDER MARCET.

</div>

St. Mary Axe, Oct. 16, 1804.

Extract of a Letter from Mr. Maunoir, furgeon at Geneva, dated 5th September, 1804.

" Mr. F. of Geneva, aged forty years, had loft his hearing, feveral years ago, in confequence of a long affection of the pofterior part of the internal fauces, and it was evident, that, in this cafe, the euftachian tubes were obliterated. In condenfing the air in his mouth and noftrils, he was totally unable to diftend the membrane of the tympanum, a thing which is very eafy to do for any one who enjoys a perfect fenfe of hearing. He could fcarcely hear, even when you fhouted in his ears, and although fo long deaf, he had not acquired the faculty of underftanding by the motion of the lips. After perufing the paper which I lately publifhed in the Bibliothéque Britannique, Mr. F. conceived that he might be one of thofe in whom the hearing could be recovered by the perforation of the

<div style="text-align:right">the</div>

(marginal notes) Introductory letter.

A gentleman aged 40 years had been feveral years extremely deaf.

the tympanum. On the 14th of July laft, I performed that operation on his right ear, in prefence of Mr. Jurine, with a very fmall trocar, about one eighth of an inch in diameter. At the inftant that the inftrument was withdrawn, we fpoke to him in a low voice; but inftead of anfwering, he remained immoveable in his chair, and feemed ftupified.—Then he ex-

claimed "For God's fake gentlemen, don't cry out fo loud; you hurt me!" I then began to walk about the room,—The fqueaking of my boots made him fhudder and jump in his chair; and he covered his ear with his hand. The fnapping of the fingers quite diftracted him, and feemed to produce upon him an effect fimilar to that of a piftol fuddenly fired in the ear of a perfon not aware of it. When fpoken to, though in a whif-per, he complained that we fpoke too loud. Yet in fpite of this exceffive fenfibility to very inconfiderable noifes, there were other founds, not lefs diftinct, which he did not hear, even at a very little diftance; fuch for inftance as the *ticking*

of his watch. So that his hearing feemed to be fometimes too dull, fometimes too acute; becaufe, no doubt, the organ had loft the faculty of adjufting itfelf to the different modulations of found. A week after this, however, Mr. F. had loft that exceffive fenfibility which rendered acute founds almoft in-tolerable to him, and in fact he had already *again learnt* to hear. He then wifhed to have the other ear perforated in the fame manner. I performed this operation, but without effect.—Twenty days after this fecond operation, Mr. F. called upon

me. I examined his ears in a fine fun fhine, and could fee in the right ear the membrane of the tympanum traverfed in its exterior part by a fmall cicatrix, having in its center a fmall hole which was hardly perceptible. Yet the hearing was fcarcely diminifhed fince the firft operation. Fearing that this fmall aperture might clofe itfelf entirely, Mr. F. defired that I would perforate again the membrane of that ear, an operation which was repeated without giving him any pain, but not without a flight increafe of fenfibility in his hearing. I after-

wards examined the left ear, and having placed the meatus in the direction of the fun's rays, I diftinctly perceived a falfe membrane adhering to the whole circumference of the paffage, and perfectly fimilar, in its form and fituation, to that of the tympanum, the diftance between the two membranes being fcarcely one fixth part of an inch. I took off at once this

                                                      falfe

falfe membrane with a pair of tweezers, and difcovered behind it, the tympanum, quite found and entire. I thought it probable that, in the fecond operation, I had only touched this falfe membrane. I immediately perforated the true one, and was furprized (as well as my brother who attended this operation), to find that, although Mr. F. had already learnt again to hear with the right ear, yet the reftoration of his hearing in the left, occafioned in him the fame effects of wonder, and of exceffive fenfibility to the leaft unexpected noife, which he had experienced after the firft operation. This inconvenience, however, has not been of longer duration in this ear, than it had been in the other."

Repetition of the operation with effect and fimilar phenomena.

III.

Letter from Mr. TIMOTHY SHELDRAKE, expofing the Errors of M. Tingry refpecting Copal Varnifhes; with fome farther Inftructions concerning the fame.

(Received Oct. 16, 1804.)

To Mr. NICHOLSON.

SIR,

M. TINGRY, profeffor of chemiftry, &c. in the academy of Geneva, has publifhed a treatife on the art of making and applying varnifhes; with new obfervations and experiments on copal, &c. &c. A tranflation of that work has lately appeared here, and at p. 157 of that tranflation I find the following paffage.

Tingry in his art of making varnifhes—

" Mr. Timothy Sheldrake mentions camphor as a medium for diffolving copal in effence* and alcohol. He gives alfo another procefs, in which ammonia (volatile alcaline fpirit of fal ammoniac) in the proportion of an eighth part of the effence employed, is fubftituted in the room of camphor.

" *Exper.* I. Of the three proceffes which he defcribes, I repeated two : thofe which regard the folution of copal in effence and in alcohol by a mixture of camphor. *That with effence did not fucceed.* The author himfelf announces that he

repeated Sheldrake's proceffes for diffolving copal in effence and did not fucceed ;—

* Throughout the book this word is ufed to defcribe what we call fpirit of turpentine.

always

always failed, except when he obtained the effence from apo-
thecaries hall. It appears that this effence had by chance all
thofe effential qualities which we endeavour to give it by time,
and ftill more fpeedily by the influence of light.

nor with alcohol. " *Exp.* II. The fame experiment repeated with pure alco-
hol *was attended with too little fuccefs to make the refult be con-
fidered as a varnifh.* The alcohol appeared milky, and the
copal formed at the bottom of the veffel a mafs which did not
feem to have decreafed in volume. Next morning the inter-
pofed part of the copal, which altered the limpidity of the al-
cohol, was precipitated, and adhered to the fides of the glafs.
The procefs I have defcribed for fpirituons tincture of amber
would give more hopes of fuccefs without any intermediate
fubftance."

He infers that the compounds muft be imper- fect and not varnifhes. " In regard to the means propofed by the medium of am-
monia, the faline nature of that liquid, *if the procefs fucceeds,*
will not admit of the product being placed in the clafs of var-
nifhes deftined for delicate painting. *It is a kind of faponace-
ous compound, the ufe of which is not to be recommended in fuch
cafes.*"

Obfervations in reply. The paffage above quoted undoubtedly relates to a paper
which I communicated feveral years ago to the Society for the
Encouragement of Arts, &c.

This paper was wholly tranfcribed * into fome periodical
publications, and parts of it into others; it may therefore be
neceffary to obferve that when reference is made to the original
paper, it muft be underftood to mean as it was printed in the
Society's Tranfactions, and not to any imperfect extracts that
may have been made from it.

Mr. Tingry ought not to have oppofed *fpeculation* againft *facts.* Mr. Tingry fhould have been fenfible of the danger in op-
pofing a conjecture or opinion *purely fpeculative,* to what is de-
fcribed as a *fact that had been demonftrated by experiment.* Since
it is more probable that the conjecture fhould be unfounded, or
the opinion untrue, than that a man fhould forfeit his reputation
with the public by relating that, as having been demonftrated
by experiment, which the perfon who contradicts him fays
cannot, in the nature of things be true, even if the *procefs
fucceeds:* As it is the intention of your valuable Journal to pro-
mote the caufe of fcience, and as that caufe will be as effect-

* See our Journal, I. 250.

ually

ually ferved by the detection of errors, as by the promulgation of new facts, I truft you will favour me by inferting the following obfervations on thofe paffages which I have quoted from Mr. Tingry's work; beginning with that in which he attempts to demolifh one part of difcovery (if it may be fo called) without ufing one experiment as the inftrument of deftruction.

" In regard to the means propofed by the medium of ammonia, the faline nature of that liquid, *if the procefs fucceeds,* will not *admit of the product being placed in the* CLASS OF VARNISHES, deftined for delicate painting; it is a kind of *faponaceous compound, the ufe of which is not to be recommended in fuch cafes.*"

In oppofition to all this I have but a fimple fact to produce: viz. It is many years fince I diffolved copal by the procefs alluded to; and actually varnifhed feveral pictures with the folution fo prepared: thofe pictures appeared to fhine, or bear out, as artifts call it, as well as if they had been covered with any other varnifh; the colour of the varnifh has not changed in the leaft, which moft other varnifhes would have done in the fame time; they have ftood in a fmoky houfe in London the whole time, have been repeatedly wafhed with warm water, and cleaned with fpirit of turpentine; and notwithftanding all this, they ftill preferve their gloffy appearance undiminifhed. If a folution which is proved by the experience of many years to poffefs thefe properties in an eminent degree, does not deferve to be admitted into the clafs of varnifhes, it is not eafy to tell to what fubftance Mr. Tingry will grant that diftinction.

Inftances of pictures varnifhed with the copal many years ago, and now in perfect and excellent condition.

Having faid thus much for the fact, we may venture to proceed to the theory: When ammonia and copal were mentioned, the combination of both thofe fubftances with the fpirit of turpentine appeared to Mr. Tingry to be fo much a matter of courfe, that he took it for granted, and drew all the conclufions from it without making a fingle experiment to prove the reality of the fact, had he done fo, even he, with the ftrong prejudices he evidently has upon this fubject, might have *doubted if the ammonia entered into the compofition at all;* to me it appears, after attending carefully to the phenomena of the folution, that it *does not;* but without determining pofitively on the fubject, I fhall defcribe thofe phenomena, and leave your readers to judge for themfelves.

The ammonia probably forms no part of the compound; but this is of no practical confequence.

The

The copal muft be grofsly powdered and put into a conve-
nient glafs veffel; the fpirit of turpentine and ammonia muft
be well fhaken together, and the mixture poured upon the co-
pal; the veffel clofed, and fo much heat applied as fhall, as
fpeedily as poffible, make the fpirit boil fo flowly that the bub-
bles may be counted as they rife to the furface; if this is fkil-
fully performed, the folution is feen to take place in the follow-
ing manner; the mixture has a milky appearance when firft
applied to the copal; the upper furface firft becomes clear, the
cloud gradually fubfides, and attaches itfelf to the copal, which
fwells and becomes opaque; a portion of it in a diffolved ftate
gradually exudes with the air bubbles, and diffufes itfelf in the
fpirit, and this continues till it has taken up as much as can be

fufpended in it; no more is then diffolved: It is on the fkilful
management of thefe circumftances that the fuccefs of the
procefs depends; for, if lefs heat than the degree required be
given, the folution ftops; if more, the bits of copal inftead
of being opaque and adhering together at the bottom of the
veffel, refume their tranfparency, and are agitated feparately
by boiling of the fpirit; and when either of thefe accidents
happened, I could never afterwards diffolve the fame portion
of copal either in the fame or any frefh portion of fpirits. If
I might venture to explain thefe phenomena, I fhould fay, that
the ammonia gradually quits the fpirit of turpentine, and unites
with the copal, and reduces it to fuch a ftate, that with the
affiftance of the peculiar degree of heat that I have defcribed,
the fpirit of turpentine is enabled to diffolve fo much as will
form a varnifh; but, if either more or lefs heat is applied, the
copal cannot be diffolved, and then the operation is abortive.

Whether this explanation be fatisfactory or not is of fmall
confequence, the exiftence of pictures in my poffeffion, which
were varnifhed with this folution many years ago, and ftill re-
main more perfect than they would have been if covered with
any other varnifh, is fufficient to prove that all Mr. Tingry has
faid on this fubject has not the fmalleft foundation in truth.

I had examined all the books which I had accefs to, without
getting any information refpecting the folution of copal for the
purpofe of making varnifh, till in Lewis's tranflation of Neu-
man's works I found the following, Vol. II. p. 27. "Solutions
of copal have been greatly efteemed as varnifhes, and the me-
thod of making the folution kept a fecret in particular hands:

Junker

Junker informs us that it *readily succeeds, if spirit of sal-ammo-niac, mixed with a due proportion of oil of spike or oil of tur-pentine, be ufed as the menftruum."* Upon this (int I pro-ceeded till I had difcovered the procefs I have defcribed ; but, though the varnifh anfwered every purpofe I could wifh, the trouble of making it being great, and the failure of the procefs fo frequent that I determined to feek for fome more convenient method.

Having found that copal, when powdered and rubbed in the mortar with a fmall quantity of camphor, became foft, and in a few minutes, united in one coherent mafs, I conjectured that by means of camphor, copal might be diffolved in alcohol, and upon trial found that it might be fo diffolved, and rendered fit for the purpofes of varnifh ; this I did, not by way of experi-ment, but in confiderable quantities and at various times ; fo often, that I am authorifed to fay, that if the materials are good, and the procefs I have defcribed in the Tranfactions of the Society for Encouragement of Arts be followed, there can be no difficulty in making a colourlefs fpirit varnifh from copal ; and as a proof that this may be done, I fend you a fmall quan-tity that I made fome time ago, and which will enable you to judge whether it deferves to be *"placed in the clafs of varnifhes."* [Invention of camphor as a better medium than ammonia.]

But as it is improper, for many reafons, to varnifh pictures with any fubftance diffolved in alcohol, I tried to diffolve the copal in fpirit of turpentine by the intervention of camphor, and fucceeded ; not fo eafily indeed as in alcohol, but always with the certainty of fuccefs, when the precautions I have de-fcribed were obferved; this has been uniformly the cafe with myfelf for feveral years, as well as feveral artifts who have made the trial ; and it has now been proved by many years ex-perience that copal diffolved, by means of camphor, in fpirit of turpentine, is as colourlefs as the refin itfelf and perhaps as durable, fince pictures that have been varnifhed many years with it have been preferved without the leaft change even in the colour of the varnifh, which would not have been the cafe had any other fubftance been ufed. [Sp. of turpen-tine preferable to alcohol.] [This varnifh is alfo excellent.]

As Mr. Tingry's *experiments* lead to very different conclu-fions, I am compelled to fuppofe, either that they were inac-curately made, or that the copal, camphor, alcohol, and *

fpirit

* Mr. Tingry himfelf, *as he fays,* tried to diffolve copal in fpirit of turpentine, and the procefs did not fucceed : his notion that the

fpirit

spirit of turpentine in his country are very different from the substances which are sold by the same names in this.

My intention in communicating those papers to the Society was to assist those who are engaged in the practice of the arts, by making public a method of making a varnish that must be valuable to them: and I have been induced to trouble you with this, in hopes that, by printing it, you may prevent those false notions, which might be formed upon Mr. Tingry's experiments from gaining credit; to which you will allow me to add such hints and improvements as may prevent disappointment to those who undertake to prepare this varnish for themselves instead of depending upon those who make it for sale; this every artist should do if he resolves to have it perfect; for, though I do not mean to suppose that every maker of varnish will substitute inferior articles, yet it is certain that where the price of one article is higher, because it is really valuable, and difficult to make, many dealers will substitute something *inferior* that produces more profit to himself, without considering whether it may not be injurious to those who are to use it.

I had several times given some of my varnish to an artist, who liked it so well that he requested I would instruct him to make it himself, instead of making frequent applications to me for it: I procured him the necessary apparatus, and gave him a written account of the process, with such verbal instructions as I could add to it, and left him to procure his own materials and proceed to work by himself; he did so, and did not succeed in dissolving the copal: he tried a second and third time with no more success; he then brought his materials to me, requested that I would try if I could dissolve them; which experiment would determine whether his materials were bad, or whether there was any defect in his manner of attempting the process: this I resolved to undertake; using the same spirit

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spirit I used had *by chance* obtained those powers for dissolving copal which *he endeavours to give it by time*, &c. is perfectly ridiculous. You and your English readers will know that in saying I did not succeed when I used spirit of turpentine from the colour shops, but always did when I got the spirit from Apothecaries Hall, I meant that at the latter place I always obtained the best article of the kind, instead of the inferior articles which are sold by the *same name* in other places, though totally unfit for this purpose.

and

and copal which he had thrice attempted in vain to diffolve; and at the fame time fubjecting another portion of my own materials to the fame procefs in every particular : I fucceeded in both inftances : the only difference was that the folution which I produced from the materials which had been tried by my friend was not fo vifcid as that which was obtained from the frefh materials.

As the procefs which I have ufed for fome time paft is dif-ferent from and better than that which I communicated to the Society, and has been publifhed in their Tranfactions, I truft you will excufe me for defcribing it here; it is fuch an one as may be fuccefsfully ufed by any perfon upon a common fire, without any danger, and which any man of fcience, or who may chufe to adopt in a large way, may eafily modify fo as to anfwer his purpofe. **Improved procefs.**

Provide a ftrong veffel made of tin or other metal, it fhould be fhaped like a wine bottle and capable of holding two quarts; it will be convenient to have a handle ftrongly rivetted to the neck; the neck fhould be long and have a cork fitted to the mouth, but a notch or fmall hole fhould be made in the cork, that, when the fpirit is expanded by heat, a fmall portion may force its way through the hole, and thus prevent the veffel from burfting. **Safe and eafy method of making copal var-nifh.**

Diffolve half an ounce of camphor in a quart of fpirit of tur-pentine, and put it into the veffel ; take a piece of copal the fize of a large walnut, reduce it to coarfe powder or very fmall pieces; put them into the tin bottle, faften the cork down with a wire, and fet it as quick as poffible upon a fire fo brifk as to make the fpirit boil almoft immediately; then keep it boiling very gently for about an hour, when fo much of the copal will be diffolved as will make a very good varnifh ; or, if the operation has been properly begun, but enough copal has not been diffolved, it may be again put on the fire, and by boiling it flowly for a longer time, it may be at laft brought to the confiftence defired.

The rationale of the operation I believe will be nearly as follows : copal and camphor have fo ftrong an affinity for each other, that when feparately powdered and then rubbed toge-ther, the copal abforbs the camphor, fwells, foftens, and be-comes one coherent mafs; nor when once united do they eafily **Explanation of the procefs;**

<div align="right">feparate</div>

separate again *. When the camphor is diffolved in the spirit, the fmall bits of copal thrown into it and inftantly made to boil, the motion of ebullition keeps the camphor fufpended, moves the copal about in the fpirits, the furface of each bit of copal is diffolved, and the whole of it foftened and enlarged, and by *—and why Mr.* continual boiling may be diffolved; but my friend did not pro- *S.'s friend failed.* ceed in this way; he reduced his copal to a very fine powder, thinking, perhaps, that would facilitate the folution; he then added the camphor, diffolved in the fpirit of turpentine, but fearful of giving too much heat, fet it by the fide of his fire till it was warmed, then moved it into a warmer place, and at laft made it boil; the confequence was that the camphor † left the fpirit and united with the copal to form one folid coherent mafs at the bottom of the veffel; the thicknefs of this mafs prevented the heat from penetrating to the fpirit fo foon as it otherwife would have done; and, when at laft it did boil, the camphor had entirely left it, and of courfe it was impoffible that any folution fhould take place.

Three times was the folution attempted in this manner, but when on a fourth trial by myfelf a fufficient quantity of cam-phor was added and the boiling heat immediately applied, the folution went on, but not fo favourably as in my other attempt, becaufe the copal was united into one mafs at the bottom of the veffel, and no more could be expofed to the action of the folvent than the furface; but, in the other experiment the copal was broke into very fmall bits and the furface of each expofed to the full action of the folvent; of courfe a greater quantity was diffolved in the fame fpace of time.

The inftance of failure here mentioned might have been urged againft the procefs. Had this gentleman been fatisfied with the experiment he tried by himfelf, he might have reported, from his own expe-rience, that copal could not be diffolved by the procefs I had defcribed; and yet in point of fact, nothing could have been more untrue: from this it may be fairly deduced that when perfons who are unufed to fuch experiments do undertake them they fhould be fcrupuloufly exact in following the directions

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* Six months ago I reduced fome copal to this ftate; it has re-mained ever fince expofed to the open air in a hot room, and ftill re-mains foft, the particles adhering to each other.

† When the fpirit of turpentine was poured off from the copal, it was perfectly colourlefs, and by the fmell did not in the leaft in-dicate the prefence of camphor.

given

given to them, and on any apparent failure, refort to thofe whofe experience may enable them to detect any fallacy there may be in the proceeding.

As it is well known that folution of copal is the moft valu- Remarks and able of thofe varnifhes which can be ufed for the finer works of cautions. art, and as it is proved that by the procefs I have defcribed, thefe folutions may be eafily made by any one in almoft any fituation, it only remains to mention fome cautions which are neceffary to enfure fuccefs.

Firft, That the fpirit and camphor fhould be united before the copal is added.

Secondly, That the copal fhould be grofsly powdered before it is added to the fpirit, the whole fhould then be made to boil as *quickly as poffible*, but not *violently*, and that ebullition fhould be kept up regularly till the folution be compleat.

There feems to be no difadvantage in ufing too much cam- Effects of an phor if it fhould fo happen; but it *is* difadvantageous to put overdofe in too little copal;' becaufe a certain quantity of fpirit will of camphor or of copal. take up a given quantity of copal; and if more is added than will be diffolved, the fuperabundant copal will abftract more camphor from the liquid, and thus render the folution more tedious and difficult, and fometimes, perhaps, entirely pre- vent it.

The advantage that attends the ufe of glafs veffels is that the Glafs veffels and folution may be feen all the time it is going on; the difadvan- thofe of metal compared. tage is that perfons who are not accuftomed to fuch purfuits, may inadvertently give too much heat, and thus fubject them- felves to ferious accidents; but by ufing veffels of metal with the precautions I have given, no accident can happen; the only difadvantage is that they cannot fee what they are doing; this is of fmall confequence, as it may always be determined with fufficient accuracy by the ear whether the fpirit boils, and to what degree; and as no accident can happen, without very grofs neglect, from the ufe of them, they certainly fhould be preferred.

As I have written more than I at firft intended, I fhall con- Conclufion. clude with a requeft that you will ufe your difcretion in omit- ting any part or rejecting the whole of what I have written, if you think the publication will not anfwer any ufeful purpofe. I fend with it a fmall quantity of copal diffolved in alcohol, and

another

another in fpirit of turpentine, which will enable you to fatisfy yourfelf and your readers whether thefe folutions *deferve to be placed in the clafs of varnifhes,* and remain

Your moft obliged fervant,

T. SHELDRAKE.

No. 5, Montague-Street,
Ruffel-Square.

IV.

Reply to Mr. DALTON, *on the Conftitution of Mixed Gafes. By Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,   *Middlefhaw, Oct.* 16, 1801.

Character of Mr. Dalton's reply.

I DID not attack Mr. Dalton's opinions openly, until he had invited me to do it; the attempt has been made on my part purfuant to his requeft; and he has replied, but in a manner which feems better calculated to amufe the fuperficial reader, than to convince the reafoner: For he treats the fubject, fometimes with acrimony, and fometimes with ridicule; in the mean time his arguments are but few, and thefe appear to be negligently conducted. A bare infpection of his own letter will fupport the two firft charges, and I will now enter upon the defence of the third.

Mr. Dalton begins by pronouncing the diftinctions which I make betwixt a mechanical hypothefis and theory, to be nugatory; notwithftanding the caufes of thefe diftinctions are fully ftated in my Strictures. Now controverfy is of a nature which always obliges a man to point out the fallacy or futility of his opponent, when he can do it with fuccefs; but this has been neglected by Mr. D. in relation to my diftinctions, and I leave him to draw the inference which refults from thefe obfervations.

Mr. D.'s demonftration reviewed.

Mr. D. proceeds, in the next place, to demonftrate the fundamental propofition of his hypothefis; namely, the mutual penetrability of gafes which do not attract nor repel each other. For this purpofe he remarks, that oxigen repels oxigen, but

not

not azote.   This affumption is fairly allowed to be a poftulate by Mr. Dalton; and he then proceeds to obferve, that this being admitted, it follows, that if a meafure of oxigen be put to one of azote, the oxigen finding it, *viz.* the azote, porous, enters its pores, and *vice verfa*, &c.   But here a fecond and an effential poftulate is flily introduced into the fyllogifm, under the form of an inference; becaufe no ftrict logician will venture to fay that a body, A, muft be porous of neceffity, becaufe it repels a body B ; on the contrary he muft conclude, that this miftake in his art deftroys the demonftration, and does not retrieve the doctrine from the imputation of being an hypothefis.   The fimile of the philofopher, cottager, and fieve, may be calculated to promote ridicule, but it is badly chofen; for it is impoffible to demonftrate the propofed problem from Euclid's Elements, becaufe this work confines its fpeculations to abftract figures and magnitudes.   But a mathematician might demonftrate the fame to a brother mathematician, from the elements of the mechanical philofophy; and he ought to have his demonftration ready upon demand, which is not the cafe of Mr. D. in the inftance, of his fundamental propofition.

Mr. D.'s fimile incorrect.

It is a matter of fome furprife, that Mr. D. has drawn his demonftration from the mutual affections afcribed by him to oxigen and azote; feeing I had profeffedly attempted to fhew the fallacy of his fecond and effential poftulate, by afcribing the fame affections to aqueous vapour and the permanent gafes of the atmofphere.   This unfortunate choice obliges my opponent to be content with a diftant fide-view of the arguments which have been brought againft him; whereas a ftricter obfervance of the path * chalked out for him in my Strictures, would have given him an opportunity to confront them, and defend his atmofphere of fteam.   The thing however has turned out, for fome reafon or other, as it is here reprefented; and Mr. D. arrives very briefly at the following fingular conclufion; that though the demonftration of my propofition refpecting air and vapour, be rigid, the previous data are not correctly affumed.   Now this is the point which I endeavoured to eftablifh in the propofition alluded to above; for his fecond poftulate makes one of the incorrect data ; in fact, they are all

The fecond poftulate falfe in the inftance of vapour and air.

* Phil. Journ. Vol. IX. Page 33.

his own except this : If a particle of vapour can pafs freely through the air, a fecond can alfo fucceed it at any given diftance. My antagonift will certainly grant me this conceffion ; becaufe the falvation of his aqueous atmofphere obliges him not to fufpect it. Should this indulgence be extended to my Strictures, the demonftration in queftion muft be re-examined with greater diligence by Mr. Dalton; for if it cannot be refuted, it will manifeftly do away his atmofphere of free vapour, by proving the abfurdity of his fecond poftulate in the cafe of air and water; and the fame obfervation may alfo be extended to all the permanent gafes which are not abforbed by water; becaufe no fluid of this defcription forces water to pafs through its pores by preffing upon it.

The ufe of the contemplative analyfis. After all, if we may judge from Mr. Dalton's reply, he appears not to comprehend my arguments on this fubject : they have however nothing of novelty in them; for the beft rea_foners in all ages have not hefitated to admit a hypothefis is for the fole purpofe of difcovering its worth, by comparing the confequences refulting from it with known facts. This kind of argumentation was in great efteem with the Greek mathematicians, and Pappus of Alexandria has defcribed and recommended this fpecies of logic under the name of the contemplative analyfis.

Facts oppofing the hypothefis. Mr. Dalton fays, he is acquainted with no facts that contradict what I call his experimental probabilities. I can furnifh him with one to which perhaps he is a ftranger, and remind him of another, which he will not difpute : it will therefore be his bufinefs to reconcile them to the hypothefis of an aqueous atmofphere.

An experiment by Mr. Kirwan. Firft, Mr. Kirwan, in the courfe of his ftatical experiments on air, conftantly found a given bulk of it to be lighter, cæteris paribus, when De Sauffure's hygrometer was at 90°, than when the atmofphere was lefs humid. Now according to Mr. D.'s hypothefis, all the permanent gafes, of thi given bulk, had the fame weight both in the dry and moift ftate of the air; becaufe they were of the fame denfity on account of the cæteris paribus : but the aqueous vapour was moft abundant when the weight of the aggregate was leaft ; that is, the weight of vapour diminifhes, while its denfity increafes. Will Mr. D. admit the truth of the preceding conclufion, to the detriment of his own doctrine; or will he refute the inference, by denying the fact?

4   *Second,*

Second, If the latter alternative fhould be preferred, the following experiment, with which Mr. D. is already acquainted, remains to be explained by the principles of his hypothefis, due attention being at the fame time paid to the axioms of Dynamics.

A moift bottle, which I had found would contain 7794 grains of water of the temperature of 59°, was placed in water heated to 126°, having its mouth elevated about two inches above the furface. This aperture was covered by my hand ; which was occafionally removed for an inftant, that the warm air might efcape. At the end of two minutes the bottle was inverted in cold water, the mouth of it being firft fecured in the manner defcribed above; in which fituation the temperature of it was again reduced to 59°. The air remaining in it after refrigeration, occupied 6172 of the 7794 parts conftituting the capacity of the bottle. From this fact I infer, that 6172 parts of air of 59°, will occupy 7794 fuch parts after being raifed to 126° in contact with moift glafs. But 6172 parts of dry air of 59°, will only occupy 6992.C6 parts, after being heated to 126° in a dry tube: Thus it appears that the prefence of water augmented the bulk of the air which I ufed ; how is this difficulty to be explained ? We have no ftopple of mercury in this experiment, by help of which Mr. D. explains the appearances of the manometer; confequently, as nothing was in the way to prevent the free egrefs of the vapour, it expanded itfelf in the bottle, and acted on the air contained in the fame, juft as it does in the atmofphere. But it increafed the bulk of this air, which could not be effected on Mr. D.'s principles, otherwife than by diftending the pores of it. Now his own tables of the force of vapour will fhew this to be impracticable, if there be any truth in the axioms of Dynamics. On the contrary, nothing occurs in the two laft experiments repugnant to chemical union ; and as water makes a part of the atmofphere, it muft be attached to air by the mediation of affinity.

I have now done fomething more than anfwer all Mr. Dalton's objections : the demonftration of his fundamental propofition has been fhewn to be no demonftration at all ; his fecond poftulate has been proved to be falfe, by his own data, in the cafe of air and vapour ; laftly, two fact have been advanced which are inexplicable by my opponent's prin-

ciples:

ciples : After perufing thefe objections, can he blame me for rejecting his hypothefis. As for Mr. D.'s remarks on my theory of mixed gafes, they require no anfwer; becaufe he profeffes not to underftand it. I am willing, however, to fubmit the merits of it, as well as its claim to the title of a theory, to thofe mechanical philofophers who happen to comprehend it.

<div align="right">JOHN GOUGH.</div>

V.

On the apparent Size of the horizontal Moon. In a Letter from Mr. Ezekiel Walker.

To Mr. NICHOLSON.

DEAR SIR,

Enlarged appearance of the horizontal moon.
WHEN the full moon is juft rifing above a clear horizon, fhe is an object that pleafes every eye: But at the fame time that fhe pleafes the eye of the philofopher, fhe embarraffes his reafon, to affign the caufe of her apparent magnitude's being fo much greater near the horizon, than at higher elevations.

Attempts to account for it.
When men in former ages began to reafon on this phenomenon, they imagined that the angle fubtended by the moon, was really increafed, by a refraction of her rays, in paffing through the vapours contained in the air, near the earth's furface. But when it became known that fhe fubtended the leaft angle at the eye, when her apparent magnitude was greateft, philofophers faid that the eye was impofed upon by the long feries of objects interpofed between the eye and the extremity of the fenfible horizon. And after it was difcovered that the fame phenomenon was obferved at fea, where no land objects could be feen, they blamed the clouds for deceiving them; and when the clouds flew away, the fpirit of inquiry flew after them, to feek for information in the apparent concavity of the fky.

Such were the erroneous opinions maintained by men of the greateft celebrity, both in ancient and in modern times. To mention any more of them would be ufelefs labour: a fingle quotation from Dr. Smith's Optics will, I prefume, be quite fufficient to fhow how little was known of this matter at the time when he wrote.

<div align="right">After</div>

After the Profeffor had finifhed his celeftial explanation of this phenomenon, he juftly acknowledges that, at different times, the moon appears of very different magnitudes, even in the fame horizon, and occafionally of an extraordinary large fize, which he is not able to give a fatisfactory explanation of. *Smith's Optics*, Vol. I. pa. 63, &c. *Remarks*, pa. 53.

It is really aftonifhing, that this phenomenon fhould have remained fo long without an explanation founded on a better principle than that of mere opinion. That the dimenfions of the pupil of the eye alter by the ftimulus of light, has long been known; but I believe it ftill remains to be proved, that the picture of the moon formed upon the retina is not permanent, but varies as the dimenfions of the pupil vary. The following experiments will, I hope, remove every doubt refpecting this law of vifion.

To imitate the eye upon a large fcale, I took an achromatic lens of 1.6 inches in diameter, and 17 inches focal diftance, to reprefent the cryftalline humour. This I fixed upon a ftand in a perpendicular direction, with a moveable white fcreen behind it for the retina, to receive the image of a lighted candle, which ftood before it, at the diftance of 40 inches. To imitate the pupil, I took three cards, and numbered them 1, 2, and 3, In No. 1, 1 made a circular hole half an inch in diameter, in No. 2 a hole of $\frac{3}{16}$ of an inch, and in No. 3, a hole of $\frac{2}{16}$ of an inch.

After I had moved the fcreen until the image of the candle appeared diftinctly upon it, I found that the length of this luminous picture meafured 1.82 inches.

When the card No. 1, was applied clofe to the lens, with the centre of the hole againft the centre of the lens, the inverted image of the flame of the candle, upon the fcreen, meafured 1.46 inches.

When the card No 2, was applied to the lens, in the fame manner as No. 1, the image of the flame meafured 1.3 inches.

And with No 3, the picture of the flame meafured only 1.2 inches.

Thefe experiments fhow us, in the cleareft manner, the caufe of that variation which obtains in the apparent magnitudes of objects.

That

‹   ‹

That the pupil contracts as the quantity of light which falls upon the eye increases, is well known.

And that the magnitude of the picture of a luminous object upon the retina, decreases as the pupil contracts, is evident from the experiments.

Therefore, the magnitude of the picture upon the retina, decreases, as the quantity of light falling upon the eye increases.   •

Hence the apparent magnitude of the moon is greatest at the horizon, and decreases as she ascends; for the magnitude of her picture upon the retina is inversely as the quantity of light which she gives us.

and to the sun. The sun appears larger at the horizon than at higher altitudes, for the same reason; and terrestrial objects seen through a mist appear larger than in a clear day, in consequence of the same operation of the eye.

> I am, Dear Sir,
> With much respect,
> Your humble Servant,
>
> ‹   E. WALKER.

Lynn, Oct. 13, 1804.

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VI.

Description of the Ship Economy, 200 *Tons Measurement, built on the improved Construction of Mr.* J. W. BOSWELL. *

THE plan adopted in the formation of this ship is that designed for large ships of 500 tons and upwards, and the third mentioned in my specification †.

Its external appearance is nearly the same as that of any other vessel of the size, and the outward planking done in the usual manner. It is the internal construction alone to which the patent relates, and that is as follows :

Description of a vessel framed in a new method. The best general idea of it will be obtained by conceiving a vessel built with timbers, or ribs, much smaller than usual, with

* For which he has a patent.

† See Vol. II. Second Series, p. 81, of Repertory of Arts.

an internal framing, fo contrived as to give every reqnifite fup- port and ftrength both to them and the entire veffel, with the leaft timber, and of the cheapeft form, and without any knee-timber.

The floor-timbers are molded feven inches, and fided fix: thefe, with four futtocks and two top timbers at each fide, form what is called a frame of timbers. Thofe fmall timbers are laid down fo that their terminations all fall out in fair lines, which are nearly the fame as the ribband lines, when below the wales. Along thofe lines infide are laid fore and aft ribs, from ftem to ftern poft, fo as to fupport the extremity of every one of the fmall ribs in the fhip. The fore and aft ribs are fix in number at each fide; one directly under the water ways, another at the level of the lower beams, and the other four placed nearly at equal diftances between thefe laft and the kelfon: each pair uniting in a breaft-hook at the ftem.

The pieces of timber which form thefe fore and aft ribs are fcarfed at their extremities with hook fcarfs, and fo placed that the fcarfs fall out in fair vertical fections of the fhip, where they are fupported, and firmly bolted to tranfverfe framings, contrived fo as to unite the greateft ftrength with the leaft obftruction, and which are five in number in the whole fhip.

Thofe tranfverfe framings laft mentioned muft be confidered as the great fupport of the veffel, and the foundations, as it were, on which all the other parts reft, as the beams of a wooden bridge are fupported by the piers. Thofe tranfverfe framings are each formed by one upper and one lower beam, two pair of futtocks, a floor timber, two pair of top timbers, and four bracing pieces; the whole connected into one firm framing, felf-fupported, independent of any other part.

The four bracing pieces form each framing into a fet of tri- angular compartments: which triangular framing gives the greateft ftability poffible, as a triangular frame cannot be made to give in, or alter its figure, by any force which is not fuffi- cient to tear its connecting parts through the timber of which it is compofed; a property which no other figure poffeffes.

Thefe tranverfe framings (befides fupporting the fore and aft ribs, and by them the fmall vertical timbers) tie and unite the veffel together acrofs fhip, fo as to give much greater ftrength than hanging-knees, whofe place they fupply, at a much cheaper rate.

The

Framing of the deck.

The framing of the deck is alfo divided into triangular compartments, as fpecified in my patent, as to preclude the ufe of lodging-knees entirely; which compartments are formed by fix pieces of timber, which proceed obliquely at each fide, from the top of each beam to the fore and aft rib next adjoining, into which they are dovetailed and bolted; long carlings from beam to beam, at each fide of the hatchways, with thefe pieces, fupport fmall ledges, on which the deck is laid in the ufual manner.

The veffel in coming round from Southampton water failed remarkably faft, and ftayed and fteered admirably well.

Advantages of this Method of framing Ships.

Advantages of the method:

1ft. Timber of lefs than one fourth of the ufual girth can be ufed, in this method, in conftructing large veffels, for nearly four-fifths of their frames.

In price of timber;

This will be a direct faving in the difference of price of fmall timber and large for the quantity ufed; for large veffels this will be confiderable, and, according to the prefent contract prices for naval timber, not lefs than from two to four pounds per load. Befides this, it is a great national benefit in another point; for, by this means, timber of half the number of years growth, or lefs, can be ufed for naval purpofes; and thus forty or fifty years, or even lefs, be fufficient to produce timber fit for the navy, inftead of the vaft period of near a century, now neceffary; by which the land will not only produce a double crop in the fame time, fit for this purpofe, but all danger be removed of there being a ftoppage of building, for want of a fupply of timber, at any future period; an event extremely probable to take place, from the increafing difficulty of getting the large kind ufed at prefent in the Royal Dock Yards.

ufe of fhorter pieces, &c.;

2d. Much fhorter timber may, in forming the futtocks, be ufed, without any danger of weakening the fhip, on account of the great fupport given to them by the fore and aft ribs, and other internal framing, before defcribed.

The advantage of this is, that it renders the compafs timber for futtocks eafier to be procured, and prevents any neceffity of ufing any timber cut acrofs the grain.

knees fuperfeded;

3d. The ufe of knees of every kind is fuperfeded by this mode of building, as the triangular framing of the decks gives

all the effect of lodging-knees, and that of the tranfverfe frames more than fupplies the fupport given by hanging-knees.

This would occafion a confiderable faving in large veffels, on account of the great price of knee-timber fit for them; which, for that of 30 feet meeting, is near ten pound per load and for the fmalleft kind, taken at the dock-yard, not lefs than 8l. 15s.

4th. Plank of half the ufual thicknefs may be ufed for lining; *thinner plank-* the great fupport given by the fore and aft ribs rendering any *ing;* ufe of infide plank, to ftrengthen the veffel, needlefs, and confining its purpofe merely to prevent ballaft, or other matters, from getting between the timbers, fo as to reft on the outfide plank.

This will alfo caufe a faving of confequence in large veffels; plank of all kinds, but particularly that of great thicknefs, being the next deareft article to knee-timber.

5th. It is probable a much lefs quantity of timber might be *lefs quantity of* ufed with fafety in this method, on account of the great ftrength *materials;* produced for the timber ufed. 1ft. By the triangular framing. 2d. By every timber having a folid fupport at each extremity. 3d. By the increafe of thicknefs from in to out all along the fore and aft ribs, being very great in proportion to the timber ufed.

6th. It is probable, veffels built in this method will laft *durability.* many years longer before decay; becaufe the ufe of fmall timber admits of a kind more fpiny and durable than the large, which is often dotard, and never lafts fo long; and alfo becaufe this conftruction admits of a free circulation of air among the timbers, than which nothing is known to contribute fo much to their prefervation. It is moreover conceived, that the timbers being prevented from working by the folid fupport each has at it extremities, will caufe the veffel to wear lefs, and at the fame time render it fafer, by diminifhing the danger of ftarting planks, or otherwife caufing bad leaks.

7th. The timber of confiderable fize ufed in this method is almoft all nearly ftraight, or of very little curvature, on account of its running fore and aft.

This kind is much eafier to procure than large compafs timber.

8th.

8th. Short-top timber and coarfe butts can be worked up to advantage, inftead of being fold for lefs than half coft, or burned; as this kind will do fufficiently well for the number of fhort ledges in the deck frames, and to fupport the lining at the floor, which are wanted in this mode of building.

9th. Veffels built in this manner will not be fo liable as others to hog, or have their backs broken, on account of the great ftrength length-ways, caufed by the fore and aft ribs.

10th. Veffels fo built will be drier, from the circulation of air before mentioned, and having the floor-lining detached from the timbers; which quality renders this conftruction particularly valuable for the fhips ufed in the Eaft and Weft India trade.

The advantages above recited relate to veffels entirely formed in this manner. It fhould be known alfo, that parts of this plan may be applied with profit. The mode of framing the decks, for inftance, might be ufed to fave lodging-knees in veffels built in other refpects in the ufual mode. Other parts of it might be applied to the ftrengthening old veffels, which, by this means, might be made to laft many years, after they would otherwife have been unferviceable.

The principles of this method of building are capable of being extended ftill further than they are in the veffel here defcribed: the triangular framing may be even adopted to the conftruction of fore and aft ribs, fo that thefe could alfo be conftructed of fmall timber, if required. Thus, by this means, the former barrier to the increafe of fize in fhips is removed, as it no longer now depends on the fize of timber ; and fhips of any dimenfions required may be formed, of any ftrength requifite, of fmall timber.

J. W. BOSWELL.

VII.

Concluding Remarks on the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

I AM forry to trouble you again on a fubject which I fear can afford but little intereft to the generality of your readers; but I muft requeft the admiffion of a few remarks upon the letter which you publifhed in your laft number from H. G.

Your correfpondent is certainly miftaken when he afferts that our methods of calculating fquares are " precifely the fame :" Both, indeed, are derived from the principles of the binomial theorem ; but I conceive that there is a material difference in our manners of applying thofe principles to prac- tice. As we find the fquare of the given number by adding a certain quantity to the known fquare of the number next be- low it, it follows, that in both methods the quantity fo added muft be the fame ; but there is a confiderable difference in the operations by which we find this number : I do it by the re- peated addition of the fmall number 2, whereas H. G. finds the fame quantity by the repeated doubling of a number, which is conftantly changing, and may poffibly confift of many figures. To fhew the difference more clearly, I will calculate feveral fquares according to the direction given in P. 150, Vol. VIII. and I will take for my example the fame which I have taken in P. 6, Vol. IX. that your readers may the more eafily form the comparifon for themfelves.

$$28261 \times 2 = 56522 = 2.28261$$
$$798684121 = \overline{28261}|^2$$
$$1$$
$$798740644 = \overline{28262}|^2$$

$$28262 \times 2 = 56524 = 2.28262$$
$$798740644 = \overline{28262}|^2$$
$$1$$
$$798797169 = \overline{28263}|^2$$

28263

On the compu-
tation of fquares
and cubes.

$$28263$$
$$2$$
_____

$$56526 = 2.28263$$

$$798797169 = \overline{28263}^{\,2}$$
$$1$$
_____

$$798853696 = \overline{28264}^{\,2}$$

$$28264$$
$$2$$
_____

$$56528 = 2.28264$$

$$798853696 = \overline{28264}^{\,2}$$
$$1$$
_____

$$798910225 = \overline{28265}^{\,2}$$

Perhaps it may be objected to my statement, that the calcu-
lator would be aware that $28264.2 = 28263.2 + 2 = 28262$
$.2 + 4$, &c. or that he muft foon difcover this law of conti-
nuation; and therefore, that he would not proceed in every
inftance to the actual multiplication of the root by 2. Such
an objection, however, will avail nothing againft my argu-
ment, for in that cafe he will no longer work by the rule
given by H. G. in P. 150, Vol. VIII.; but if he proceeds to
find the difference by addition, he muft virtually purfue the
method which I recommend: the only difference will be, that
the practice of calculation will have fuggefted to him an im-
provement which he might have originally derived from firft
principles.

I fhall not enter into the queftion, whether addition is, or
is not, a fimpler procefs than fubtraction. The prefent quef-
tion may be decided independently of it. For granting that
there is no reafon to prefer one to the other, yet the two me-
thods by which we find the firft differences of the cubes,
might be fairly put to iffue upon the number of figures ufed in
each. To find them according to the method recommended
by H. G. in p. 150, Vol. VIII. we muft fubtract the whole
of one cube from the whole of the other; whereas I find them
by the conftant addition of the fecond differences, which muft
neceffarily be much fmaller numbers. Thus to find the firft
difference between the cubes of 26561 and 26560:

According to H. G.	According to E. O.
18738432796481	2116221121
− 18736316416000	+ 159360
2116380481	2116380481

It is ftated, indeed, by H. G. that he avoids the continual
repetition of firft differences. By this we muft not underftand
that

that his method makes it unneceffary to calculate them ; for by mere comparifon of p. 9 with p. 125, it will be feen that we both ufe the fame numbers. In this inftance, as in that of the fquares, the real difference between the two methods confifts in the manner of finding thefe additional quantities. H. G. muft therefore mean, that he avoids the neceffity of calculating the table given in p. 8. Now in this I do not think that any advantage is gained. For if the firft differences are to be found feparately, I have fhewn that they can be more eafily found by the method which I have recommended ; and much expedition will be obtained by making a feparate table of them, which is impoffible if we work according to the directions of H. G. I may, with fome reafon, complain, that H. G. has not made his example a fair parallel to mine ; for I have fet down every figure which need be ufed, while he has declined printing any of the operations whereby he determined his firft differences. It is poffible, indeed, from long practice in arithmetical computations, that he may be able to caft his eye from one cube to the other, and fo find the firft differences, without copying them out on the fide of his paper for fubtraction. I fhall not dwell upon the danger of miftake in fuch a procefs : the quickeft and moft accurate eye could hardly avoid frequent errors, and the common arithmetician will find it attended by confiderable difficulty. But granting that all this difficulty could be completely conquered, ftill it would give no fuperiority to the method recommended by H. G. ; for it might be applied to mine with equal, if not greater eafe and advantage. Thus, in the example given in p. 125 ; if F were computed by the addition of C and D, inftead of the fubtraction of A from E; if we confider $I = F + G$ inftead of $= H - E$, $M = I + K$ inftead of $= L - H$, &c. we fhould have the differences computed, according to my rules. We may remark likewife, in this fhorter method of calculating, that, according to the arrangement given by H. C. himfelf in p. 129, the eye muft neceffarily pafs over two intermediate lines of figures in fubtracting one cube from the other, whereas the two rows of figures which I would have added to one another, may always be placed contiguous. This certainly is an advantage ; but at the fame time I only mention it to fhew the

grounds

On the compu-
tation of fquares
and cubes.

grounds on which I prefer my method to that of H. G.; for I ftill confider the plan which I firft laid down as the eafieft and fureft. . .

I have faid, that if the differences are confidered in p. 125 as found by addition inftead of fubtraction, the example would fuit with my method, becaufe the fecond differences are ftill additional, and will be found, upon comparifon, to be added in exactly the fame order as in pp. 8 and 9. The only variety is by making two tables: I add the fum of the two numbers which are feparately arranged under each cube. I have only to add on this part of the fubject, that H. G. has adopted my method in fome meafure; for he fays that $K = G + 6$, as well as $= 6d$. It is true (as I remarked with refpect to the fquares, that this equality would moft probably occur to the calculator; but at the fame time we muft recollect, that it is different from the rule laid down in p. 150, Vol. VIII.

I fhall not add any thing to what I have ftated in p. 5, with refpect to the miftakes to which either method is liable; but I fhall conclude by affuring you, that it is not my intention to trouble you any more on the prefent fubject. I thought it might be ufeful to publifh the letter which I firft fent you, and having done fo, it became neceffary to remove any obfcurities in it, and to anfwer any miftaken objections which might be raifed againft it: but this having once been done, there can be no occafion for further controverfy; and I have only to thank you for the indulgence which you have granted me.

I am, Sir,

Your obliged humble fervant,

E. O.

P. S. I take this opportunity to beg you will mention two or three prefs errors which occur in my former letter.

P. 9, line 20, dele the Italic e.

P. 10, line 12, $\overline{x + 1}^3$, read in both inftances, $\overline{x + 1}^2$

P. 12, line 16, 29 read 27.

VIII.

Letter from C. WILKINSON, *Efq. on Galvanifm and Electricity.*

Dublin, Oct. 8, 1804.

To Mr. NICHOLSON.

SIR,

WHEN at Liverpool a few days fince, at the Athenæum **Obfervations on** I found a paper of Mr. Thickneffe of Wigan in your valuable **galvauifm, &c.** Journal, and which has induced me to trouble you with the following obfervation. Mr. T. ingenioufly conjectures that the galvanic phenomena depend more upon the decompofition of the water employed than as to any chemical change effected on the metals. Mr. T. obferves that two metals are requifite to the production of galvanic phenomena : this is no ways the cafe, it is well known that a fingle metal fuffices, or even brain and mufcle, according to the experiment of La Grave, or nerve and mufcle according to Aldini. Mr. T. further obferves that hydrogen mixed with copper always renders it brittle. I fhould wifh to know this gentleman's authority for fuch an affertion : he alfo fets out too hypothetically as to electricity being a modification of caloric. It has always appeared to me that the galvanic phenomena entirely depend upon the difengagement of electricity from the metal undergoing a chemical change.

Galvani has afcertained that gold, filver, copper, iron, tin, lead and zinc, conftitute the feries of metallic bodies; that when two metals the moft remote in the feries are united, the moft powerful galvanic combination is formed;* thus gold and zinc, filver and zinc, copper and zinc, &c. and laftly, lead and zinc, form the weakeft galvanic combination. The difpofition to oxidation is in the inverfe order, thus zinc will become oxidated even by expofure to the air, whilft filver and gold undergo this change with the greateft difficulty.

Hence a metal which is oxidated with the greateft difficulty combined with a metal which oxidates with the greateft facility, form the moft powerful galvanic combination. The production of galvanic phenomena is always proportionate to the degree of oxidation.

All

All fubftances which are conductors of electricity, if they undergo any change as to their conducting power, fo as to become diminifhed as conductors, always in the change lofe a portion of their natural electricity. Thus metals which are the beft conductors, when oxidated, become non-conductors in this change, the combined electricity is loft.

If a plate of zinc is univerfally immerfed in a fluid which will produce chemical changes upon it, no galvanic phenomenon will be produced becaufe the metal in every affignable point, being equally acted on, the contrary ftates of electricity cannot then be produced.

If only one fide be acted on, every affignable point on that fide undergoing a chemical change, by which it is difpofed to part with its combined electricity, there will be a general tendency of the electricity in the fubftance of the metal towards the furface acted on ; the fame as if in a veffel filled with water, a fmall aperture be formed, there will be a general tendency or current of the water towards the aperture,' fo of electricity.

Dr. Wollafton has proved by very ingenious experiments, that when two diffimilar metals are placed in a fluid which will act upon one of the metals and not upon the other, when the two metals are put into contact in the fame fluid, that thus chemical changes are effected on both ; thus that gold and filver thus arranged, will be acted on by the nitric acid.—When two metals are placed in a fluid which will act upon either of them feparately, if the two metals are preferved in a ftate of feparation while in the fame fluid, chemical changes will only be effected upon one. Thus zinc and copper placed in nitrous acid and water, only the zinc will be acted on.

Thefe principles point out the neceffity in our galvanic troughs to have our cells perfectly infulated, and that there be no communication between the refpective plates.

Mr. T. has by miftake obferved that the fenfation is in proportion to the furface acted on; the experiments of the French philofophers proved that the action of galvanifm on animal fubftances is in the ratio of the number of plates employed, and not the furfaces expofed.

In all my experiments in town with my own electric battery I never fucceeded in giving the flighteft charge with my galvanic apparatus. In fome converfation I had with you, Mr. Editor, you conjectured that an extenfive feries of fmall jars would

would be the beft arrangement. When at Liverpool, I met with fuch at the houfe of Mr. Dalton, a very ingenious lecturer on electricity. The galvanic battery I employed confifted of 200 eight-inch plates, which had fufed at the time near five feet of fteel wire; this communicated a very flight intenfity to the electric battery, containing forty feet coated furface, juft fufficient to convulfe a frog, and although the electrical battery was in the beft ftate of preparation, we could not fucceed in producing any intenfity as even to affect the tongue. At another period, I fhall do myfelf the pleafure of tranfmitting to you an account of fome ingenious experiments of Mr. Dalton, on electricity, he finds a cylinder, when exhaufted of air, or when one atmofphere is condenfed into it, will not then be capable of excitation.

<div align="center">
I am, Sir,

Your's fincerely,

C. WILKINSON.
</div>

No. 19, *Soho-Square.*

IX.

Method of preventing Accidents to Horfes and Carriages, in going down Hills, by a Gripe or Clafp acting on the Naves of the Wheels of the Carriage. By Mr. W. BOWLER.*

SIR,

THE invention I have now fent you is, I think, likely to be of great ufe, and I therefore offer a model thereof for your infpection. In the firft cafe, this cart may be ftopped in an inftant, in going down the fteepeft hill with a load, without ftopping the horfes, by the carter only preffing his hand upon a lever. This plan would likewife be highly advantageous in cafe of a horfe taking fright, as the carriage may be inftantly ftopped by the brace clafping the wheels. It may alfo, with little alteration, anfwer for a broad-wheel waggon with a heavy load. By the prefent method, a coachman, whilft fit-

Apparatus to lock the wheels of carriages.

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* Tranfactions of the Society of Arts. A bounty of ten guineas was given to the inventor, and the Society have a model.

ting on his box, may confine or releafe the wheels of his car-
riage at pleafure, and prevent accidents in defcending hills, or
in managing reftive horfes.

I am, Sir,
Your humble Servant,
WILLIAM BOWLER.

Finfbury-Street, May 13, 1803.

To Mr. CHARLES TAYLOR, *Sec.*

*Reference to the Engraving of Mr. William Bowler's Gripe for
the Wheels of Carts and Carriages,* Plate X.

A B. The extent of the body of the cart. C D. The two
fhafts. E. The front ladder of the cart, part of which pro-
jects over the fhafts, and is fupported on them: the other part
is faftened to the upright piece F. G. The handle of the le-
ver, fliding betwixt two uprights, and moveable on an iron
bar and pivot at H, in the upright piece F. By preffing down
this handle, the moveable joints I K raife upwards the lower
part of another lever L, fo as to prefs againft the lower part
of the nave of the wheel, and at the fame time the moveable
joints prefs down the lever O, and caufe this lever to act againft
the upper part of the nave of the wheel; thus compreffing
the nave of the wheel betwixt a double brace, and either re-
tarding wholly the motion of the wheel, or allowing it to move
a little, as may be thought requifite. The circle of the nearer
wheel is fhown by the dotted lines P; but this wheel is not
added, as it would prevent this gripe from being clearly feen.
The handle of the lever G produces at the fame time a fimilar
effect on the further wheel R, by means of the bar of iron
which croffes the cart at H, and acts in the fame manner on
fimilar double joints at the other fide of the cart, as may be
feen at S, where the further double gripe is fhown preffing on
the nave of the further wheel. T. A fmall catch, which, by
falling into the notches or teeth of the lever, when it is preffed
down, holds both the gripes in one certain pofition.

When the wheels are to be fet at liberty, the catch T is re-
moved out of the teeth of the lever; then the lever G is
raifed above the hole V, and kept from fliding down by an
iron pin attached to the chain U, being put underneath the
lever, through the hole V.

The

The iron-work is not any inconvenience in the loading or unloading of the cart, nor occupies any room within it; and, what is of material confequence in new inventions, it may be managed by the carter with much lefs time and trouble than the common hook and drag chain. It is alfo much more fecure in its action, as it binds on both wheels at once, and gives a uniform fteady draught for the horfes, which prevents them from falling down.

X.

Obfervations and Experiments to elucidate the Operation of the Galvanic Power. By Mr. CHARLES SYLVESTER. In a Letter from the Author.

To the EDITOR of the PHILOSOPHIC JOURNAL.

SIR,

I HAVE for fome time read and admired your valuable publication, but have not before this time ventured a communication on any fubject. If you think the following thoughts and experiments worthy the attention of your readers, I fhall feel myfelf honoured if you give them a place in your Journal.

I remain, Sir,

Your obedient fervant,

CHARLES SYLVESTER.

Sheffield, Oct. 16, 1804.

The decompofition of water, or at leaft, the prefence of oxigen and hidrogen, is obferved in all the modes by which the galvanic energy is excited. I obferve that fome of your correfpondents are inclined to doubt the truth of water being a compound body, from obferving in its decompofition that the oxigen and hidrogen are given out at fo great a diftance from each other. Though thefe philofophers have more fimply accounted for fome of the phenomena by making water a fimple fubftance, their affumed data, that pofitive and negative electricity are two diftinct bodies, is by far more gratuitous than the pofition, that water is a compound. Befides this hypothefis will only account for a few of the facts, while it abounds with contradiction when applied to the reft of the phenomena of galvanifm.

Whether water be decompofed in the galvanic procefs;

—or pof. and neg. electricity be diftinct bodies.

N 2

They

They tell us of two kinds of electricity coming from the galvanic apparatus one from the zinc end, the other from the copper end: the former being pofitive, the latter negative.

Two diftinct fluids moving in contrary directions could not poffibly exift in the galvanic trough; by reafon of that fluid proceeding from the zinc meeting that coming from the copper in each of the cells, and confequently eftablifhing a conftant equilibrium, without producing any effect. When the decompofition of water is only taking place in one veffel (admitting the above objection to be of no weight) the phenomena admit of an eafy explanation by this hypothefis; but when two veffels are ufed connected by a wire arch, it cannot in any fhape be adequate; for as we have in each veffel both oxigen and hidrogen gafes, there muft alfo be both pofitive and negative electricity; thefe two contrary fluids would of courfe have to meet each other in the connecting arch, and according to another part of this theory form fenfible heat *.

The pofitive wire, if a bafe metal is always oxidated; this effect is known to be facilitated by the electrical agency. According to the principles in queftion, it would be retarded by the power of affinity exifting between water and pofitive electricity. When the wires of a galvanic apparatus are brought into a metallic folution, no hidrogen is given out at the negative wire, but the metallic oxide is reduced. Now if metallic oxides be compofed of water and the metal, the negative electricity would combine with the water, and hidrogen gas would be evolved—the very reverfe of the fact.

My reafon for being thus particular in the examination of the hypothefis of Mr. Richter and Dr. Gibbs, is to prepare the mind of the reader for fome experiments which I think will contribute to the firmnefs of the hypothefis, which fuppofes water to be a compound, and that the hydrogen is carried from the pofitive to the negative wire by the electricity.

The well-known experiment of Dr. Afh (with the plate of zinc and filver in a dilute fulphuric acid) I had occafion to vary in the following manner. I took a copper wire about eighteen inches long, and bent it in the middle fo as to form

* They are of opinion that thefe two fluids combined form caloric. They alfo hold with Dr. Prieftly that what are now termed metallic oxides are the refpective metals combined with water.

two

two parallel legs, about two inches diftant from each other. at the nearer
This I laid in an earthen difh filled with dilute muriatic acid. parts of the
(bended) wire:
I then brought a piece of zinc in contact with one of the ends:
bubbles of hidrogen were immediately given out by that part
of the copper. After bubbles had appeared for about two
inches down this leg, I obferved bubbles on the end of the other
leg,—they now proceeded down the two legs till at laft they
appeared on the part where the wire was bended. I after-
wards laid a piece of zinc at one end of the fame difh and a
piece of gold at the other. I prepared a metallic arch with
two pieces of wire, one of gold, the other zinc, foldered to-
gether. The zinc end I connected with the piece of zinc in —and fo like-
the difh, and the gold end with the gold. After the contact wife in a com-
pound conductor
had been made about ten feconds, bubbles appeared on the between gold
piece of gold. The diftance was about twelve inches. I now and zinc.
placed the pieces of gold and zinc at half the diftance, and
made the communication with the fame arch. The bubbles
now appeared upon the gold in about half the before-mentioned
time. Upon bringing the pieces ftill nearer together, the gold
gave out gas almoft inftantly, but yet in all the three inftances
I had time to obferve that the bubbles always appeared firft on
that fide the gold next the zinc.

I think from the above experiments that the difengagement Whence it is
of hidrogen from the gold does not depend upon its negative inferred that the
difengagement of
ftate, for this quality muft have been produced on the gold in hidrogen does
every one of the three inftances in the fame time, from the not depend on a
neg. ftate; but
communication being always made by an arch of the fame that the elec-
length. On the contrary, it proves that the electricity difen- tricity is com-
municated
gaged from the zinc is communicated to the gold through the through the in-
intervening liquid. If we obferve the length of time taken up terpofed liquid.
in its paffage through this medium, we fhall fee that it does not
agrees with the laws of electricity. Is it not therefore proba-
ble that when the oxigen of the water combines with the metal,
the hidrogen combines with the electricity of the metal, and a
compound of this kind obferves thofe laws confiftent with the
phenomena.

In each of the cells of the galvanic trough a quantity of this Probability that
compound is diffufed through the liquid. The moment a com- the hidrogen is
in combination
munication is made between the two ends, the electricity en- with electricity.
ters each of the copper-plates, leaving the hidrogen in bubbles

on

on the furface *. As this electrated hidrogen (if I may be allowed the expreffion) does not move through the liquid with the facility of the electricity itfelf, the neceffity of a furface of copper equal to that of the zinc is obvious. It is plain that only a proportionate quantity will be carried through the whole

feries. This fact I have afcertained in attempting to reyder the apparatus propofed by Mr. Wilkinfon and yourfelf ufeful for all experiments. The fhock is even lefs when the furface of copper is lefs than a fquare inch.

The quantity of electrated hidrogen in each cell increafes from the copper end to that of the zinc in an arithmetical pro-greffion. The electricity only exifts in its fimple form while it is paffing from the copper furface through the two metals to the zinc furface. It there combines with another portion of hidrogen, which it leaves at the next copper furface, and fo on accumulating in quantity the longer the feries it has to pafs through. In the decompofition of water by the galvanic trough the veffels in which this is performed may be confidered as one cell in the feries. The electricity feizing the hidrogen at the pofitive wire and giving it out at the negative.

XI.

Memoir on the Origin of Wax †. *By* FRANCOIS HUBER, *Member of the Society of Natural Philofophy and Natural Hiftory of Geneva.*

IT has been thought ftrange that the word wax fhould feldom occur in a book which treats of bees alone : but neverthelefs as in the courfe of my obfervations, I had not attended to the products of their induftry, I could only have repeated what had been faid by Swammardam and Reaumur, and that did not feem to me to be neceffary.

I knew that thefe infects collected abundantly upon the an-theræ of flowers, that they are acquainted with the method of

* This fact is very obvious when the copper furface is very clean. The hidrogen under contrary circumftances is employed to reduce the oxide.

† From Journal de Phyfique, &c. Pluviofe, An. XII.

opening

opening them, of gathering their duft, keeping it in the ca-
vities of their hind legs, and carrying it to their hives.

It had been obferved that the particles of this duft fwells in Examination of
water, and that, when one of them burfts, an oily liquor runs the pollen of
out, which floats on its furface, but did not mix with it ; from flowers;
thefe experiments, repeated on the duft of a great number of
flowers, it was concluded that they contain the principles of
wax, but it was admitted that thefe muft undergo a peculiar
elaboration in the body of the bee, fince, according to the
experiments of Reaumur, a flexible wax could not be made
from the duft of the antheræ.

It will be feen from feveral paffages in my work that I had The true origin
adopted this opinion : a fingle obfervation of Burnens * changed of wax re-
mained fo long
all my ideas. The true origin of wax might have been fooner unknown from
known, had there been any fufpicion that it was not already an opinion that
difcovered. I fhall now ftate how I was led to doubt, and what it was difcovered;
I have done to verify my new conjeures.

I was in Switzerland in 1793; the farmer of the eftate on Obfervations
which I refided had many bees, and the greater part of his which led to
the enquiry.
hives having been ftocked in former years, the combs with
which they were filled reached to the ftands, confequently
there was no room to confruct new ones. We remarked,
however, that the working bees carried in a confiderable quan-
tity of this fecundating powder.

There was alfo in the fame apiary fome fwarms of that
year, the hives having only been ftocked a day or two ; in
fome of them the combs were only began, in others they were
larger, but in all of them there were vacancies to fill up, and
much work to do. We obferved with aftonifhment that the
bees of thefe fwarms did not carry in the pollen, and that,
neverthelefs, they worked with activity in the conftruction of
new combs, and in lengthening thofe already commenced.
Where, therefore, did they procure materials for their edifices?
After thefe obfervations, we fufpected that it was not from the
duft of the ftamina, and that they had a very different ufe for
it than that for which it was believed to be intended. We
however found that it was not impoffible to explain thefe ex-
traordinary facts, without abandoning the hypothefis of Reau-

* The name of M. Huber's fecretary; which deferves to be
known to the cultivators of natural hiftory.

mur,

' mur, by fuppofing that the bees of the old hives ftored up fo much pollen in their combs for their future wants, while thofe of the new fwarms did not carry it outwardly on their legs, in the infancy of their eftablifhment, becaufe they had no cells in which they could depofit it: it might be fufficient to enable them to conftruct their combs, if they were at liberty to fly to the flowers, procure their pollen, and return to their hives after having filled their ftomachs, where it muft be elaborated and converted into perfect wax. It was to obviate thefe doubts that I undertook the following experiments.

FIRST EXPERIMENT.

On Bees in Confinement with Honey alone for their Nourishment.

Exp. I.
Is pollen necef-
fary to the pro-
duction of wax?

Muft bees eat pollen to be in a ftate to produce wax? This was the firft queftion which I thought it neceffary to inveftigate: the method of trying the experiment was obvious; it was only required to keep the bees within their hives, and thus prevent them from collecting or eating the fecundating powder.

On the 24th of May, Burnens lodged a fwarm in a ftraw hive, with as much honey and water as was neceffary for their confumption, and he clofed the doors fo that the bees could not get out and the air be at the fame time renewed.

Wax from
honey alone.

At firft the bees were very uneafy, but became calm on removing the hive to a cool dark place: their captivity lafted five days; they were permitted to come out in a room, the windows of which were fhut: we then examined the hive more conveniently. We firft noticed that there was no honey left in the veffel which had been filled with it, with the fole intention of feeding the confined bees; and were more aftonifhed to find five combs of the moft beautiful wax, fufpended from the roof of the hive; they were perfectly white, and very brittle. This refult was very remarkable; however, before forming a conclufion from it, that the honey with which thefe bees were fed had enabled them to produce the wax, it was neceffary to enquire whether it could not alfo be explained in another manner.

The bees which I had employed had doubtlefs collected the duft while they were at liberty.

They might have done fo the evening before, or on the very day of their confinement, and might have enough in their ftomachs, and in the cavities of their legs, to extract from it all the wax which we had found in their hive.

But

But if it was true that it had been obtained from the fecun-
dating powder previoufly obtained, this fource was not inex-
hauftible, and the bees being unable to procure any more,
they would foon ceafe to conftruct combs, and fall into the moft
complete inaction. It was neceffary therefore to continue the
fame trial to render it decifive.

The 28th, Burnens returned this fwarm into its hive, after
having taken out all the combs; he fhut them up as before with
a frefh fupply of honey.

This experiment was not long, for on the evening of the
fecond day we perceived the prifoners working with new wax.
The next day the hive was infpected, and we found five combs,
as heavy and as regular as thofe made during their firft capti-
vity.

We afterwards repeated this experiment five times fuccef-
fively, with the fame bees, and the fame precautions: we al-
ways found that the honey had difappeared, and that new wax
was produced. This refult was fo invariable during this long
feclufion that we could no longer doubt that the honey alone
had fupplied them with all the elements of their wax, without
the affiftance of the fecundating duft.

SECOND EXPERIMENT.

*On a Hive from which Honey was excluded, and in which only
Pollen and Fruits for the Nourifhment of the Bees were left.*

I thought it would not be ufelefs to make the inverfe of the Exp. II:
preceding experiments: it would fhow me whether the pol-
len could not fupply the want of honey when the bees were
deprived of it, and enable them to produce wax.

I therefore enclofed a fwarm in a bell-glafs, in which had
been placed a comb whofe cells contained only pollen, and the
fole nourifhment of the bees was fruit.

Thefe bees did not make wax, nor did they form a fingle
cell during eight days, which was the time of their capti-
vity.

I was going to repeat this experiment, when Burnens re- Bees fed on
marked that the free bees were, in fome meafure, in the fame pollen and fruits
ftate as thofe we had confined; there being no honey at that do not produce
time in the flowers, they found only pollen, and did not work wax.
in wax.

I may

I may perhaps be asked how I was satisfied of this, toawhich I answer, bees-wax is white at firft, the cells foon become yellow, and in time, this colour grows browner, and in older hives the combs have acquired a blackifh tinge. It is therefore very eafy to diftinguifh the new cells from thofe which have been fome time formed, and confequently to know whether the bees are really making combs, or whether that work is fufpended; it is fufficient to raife the hives, and to notice the lower edges of the combs.

The odour exhaled by the hives, and the fhape of the bees, are indications by which it may always be known whether there is honey in the flowers; if they are combined, there can be no further doubt, and, particularly, if a great number of bees return to the hive, which are remarkable for the bulk and the

Wax-making bees. form of their bellies. Thofe which are filled with honey have the abdomen cylindrical; the name of wax-making bees belongs to them exclufively: the bellies of the labouring bees which have other functions, always preferve their ovoid form,

Nurfing bees. and their volume is never fenfibly augmented; the name of nurfing bees is proper for thefe.

The farmers of the neighbouring villages kept their bees in bafkets, or in cafes of different forms; and I was able to vifit a very great number without going to any great diftance from my habitation.

Operations of the bees during an intemperate fpiing. In 1793, an intemperate fpring had retarded the feparation of the fwarms; there had not been any in the country before the 24th of May; but towards the middle of June there were feveral in the vicinity of my refidence. At that time the fields were covered with flowers, the bees collected much honey, and the new fwarms worked at the wax with vigour.

On the 18th, Burnens vifited fixty-five hives; at the entrances of all of them he obferved wax-making bees. Thofe which returned to old hives, not having to conftruct cells, depofited their honey in the combs, or diftributed it among their companions; thofe belonging to the fwarms converted their honey into wax, and haftened to conftruct combs for the reception of their young bees.

It was fhowery on the 19th: the bees went abroad but brought home only pollen. The weather was cold and rainy until the 27th. We were defirous of knowing if this had prevented their working. On the 28th, all the hives were lifted:

Burnens

Burners found that the work had been stopped; the combs which he had measured on the 19th, were not at all increased, and were of a citron-yellow, nor was there a single white cell in any of these hives.

On the 1st of July the chesnuts and limes were in blossom, the thermometer indicated the 20th degree; the wax-making bees re-appeared, they carried away great quantities of honey, which, as we had before observed, was employed in augmenting the provisions of the old hives, and in enabling the young swarms to construct new combs. *—and a dry summer.*

The greatest activity was observable among them: the gathering of honey, and the production of wax continued until the middle of this month.

July 16th, the heat remained the same : the field flowers, as well as those of the chesnuts and limes, were completely withered; they yielded no more honey; their pollen alone attracted the working bees, and they collected it abundantly, but there was not any wax produced: the combs were not lengthened; those of the young swarms did not fill more than two thirds of their hives.

August 9th. It had not rained for six weeks, the heat was very powerful, nor was there any dew to allay it during the night: the black wheat which had been in flower for some days, did not offer any honey to the bees; they found only pollen.

On the 10th, it rained for several hours; next day the black wheat had the odour of honey; in fact it might be seen glittering in their expansed flowers. The bees found enough to feed them, but too little to induce them to work at new wax.

On the 14th, the drought re-commenced, and lasted to the end of the month: no more honey appeared upon the flowers, and when we visited the sixty-five hives for the last time, we found, 1st, that the bees had not produced any wax after the middle of July; 2d, that they had stored up a great quantity of pollen ; 3d, that the supply of honey was much lessened in the old hives, and that hardly any remained in the new swarms, that which they had collected in the spring having been employed in the preparation of wax; the pollen, therefore, has not this property, and no further doubt remained on this head.

This year had not been stormy, and I have since ascertained, by a great number of observations, that electricity is singularly favourable *Weather most favourable to the labour of bees.*

4   favourable

favourable to the fecretion of honey by the flowers: the bees
never collect it in greater abundance, nor is the preparation of
wax ever more active than when the wind is in the fouth, the
air humid and warm, and a ftorm gathering.

Heat too long continued, and the drought which is the con-
fequence of it, cold rains, and principally a north wind, fuf-
pend it entirely.

THIRD EXPERIMENT.

On the Ufe which the Bees make of the fecundating Powder.

Exp. III.
In the fecond experiment the bees did not touch the pollen
which I had placed within their reach, and, as its quantity was
not fenfibly diminifhed during this trial, I was induced to be-
lieve that it was not an aliment proper for them.

What is the ufe of pollen?
I alfo knew that the new fwarms were liable to perifh from
hunger in the middle of fummer, and even when the country
was covered with flowers, if a particular temperature, which
is too uncommon in our climate, did not favour the fecretion of
honey in their nectaria. What, therefore, is the ufe of the
pollen which they collect with fuch avidity during eight months
of the year, and of which they lay up fuch abundance?*

This queftion remained to be inveftigated.

I had a hive, in divifions, the queen of which was barren;
its combs did not contain any pollen, but they had much ho-
ney: the two narroweft fides of this hive were formed of panes
of glafs, through which the furfaces of the exterior combs
might be feen, and the conduct of the bees obferved.

The queen, bee taken away.
I took away the queen on the 16th of July, but to confole the
working bees I removed the firft and the twelfth combs, in
which there was not any thing to intereft them, and I fupplied
their places with two combs, the cells of which were filled
with eggs, and worms of all ages. I carefully cut away all
the cells in which pollen could be perceived, and fhut up the

* Reaumur was of opinion that the bees of a well-ftocked hive
might collect at leaft a hundred pounds of this fubftance in the courfe
of a year; but, having remarked that the weight of wax, fabricated
in the fame time, did not exceed two pounds, he concluded " that
the bees extract only a very fmall portion of the true wax from this
native wax, that the greateft part of it is required for their nourifh-
ment, and, that the reft is difcharged from their bodies in the form
of excrement."

hive

hive with a grating. My intention will be gueffed: I wifhed to know whether thefe infects could fupport their young without this fecundating powder.

The next day nothing extraordinary occurred; the bees fat on their eggs and feemed to nurfe them.

On the 18th, after fun-fet, a great noife was heard in the hive. Anxious to fee what occafioned it, we opened the fhutters, and obferved that all was in confufion: the incubation was ftopped; the bees ran over the combs in diforder; we faw thoufands precipitate themfelves on the ftand, thofe which were neareft to the mouth eagerly gnawed the grating; their intention was no longer doubtful, they wifhed to get out of their confinement.

I was fearful of deftroying them by continuing to prevent them from yielding to their inftinct, they were therefore fet at liberty: the whole fwarm came out, but the hour was unfavourable to their collecting, the bees did not go far from the hives, the darknefs, and the chillinefs of the air, foon compelled them to return, and probably calmed their agitation, for we faw them quietly reafcend their combs, and order appeared to us to be re-eftablifhed. This moment was taken to clofe the hive again.

On the 19th, we faw two royal cells begun on one of the combs of the nurfery (couvain); the evening of this day, and at the fame hour as the day before, we heard a great tumult in the clofed hive, it was in a general confufion, and we were again obliged to permit the fwarm to come out.

Other royal cells made,

The 20th was the fifth day of their captivity; we thought it had been of fufficient duration, and were alfo very impatient to examine the nurfery, and to fee what was the caufe of the periodical agitation of thefe bees: Burnens therefore opened the firft and twelfth windows, and drove the bees from the combs, fuffering them to take their flight in a room, the windows of which were fhut.

He firft noticed that the royal cells had not been continued, that they did not contain any worm, and that there was not an atom of the jelly which ferves for the nourifhment and the cradle of the larvae of the queens.

He fought in vain for eggs, for worms, and for the liquid in the common cells; all had difappeared. Had thefe worms died

—but the procefs did not go on without pollen.

died of hunger? Had we, by withdrawing the fecundating powder, deprived the bees of every means of nourishing them?

To afcertain this it would be fufficient to reftore them their pollen, and obferve the iffue. The bees were therefore again returned to their prifon, after having fubftituted young worms for thofe which had been fuffered to die.

On the 22d, we found that the bees had faftened thefe combs, and that they were again in a ftate of incubation; we then gave them fome pieces of combs in which other bees had ftored up the fecundating powder, and, the better to obferve what they did with it, we took fome of the pollen out of the cells, and laid it expofed on the ftand of the hive. In a few minutes the bees difcovered the pollen in the combs, and that which we had taken out; they took it grain by grain in their jaws, and conveyed it into their mouths; thofe which had eaten moft voracioufly re-afcended the combs, and placed themfelves, at firft, upon the cells of the young worms, which they entered with their heads foremoft, and remained there a greater or lefs length of time. One of the windows of the hive was now opened cautioufly, Burnens powdered the bees which eat the pollen, and watched them for fome hours; he obferved that the marked bees always re-afcended to the nurfery, and immediately entered the cells of the young bees.

The 23d, we found the royal cells begun.

The 24th, we drove the bees from off the young worms, and we remarked,

Ift, That all of them had the jelly, as in the common hives:

2d, That the worms had grown larger, and were forwarder in their cells:

3d, That others had been fhut up again: And,

4th, That the royal cells had been lengthened.

The 25th, we withdrew the pieces of comb which we had placed on the ftand, and found that the quantity of pollen was fenfibly diminifhed; we afterwards replaced them in the hive with other cells filled with the fecundating powder.

The 26th, the royal cells had been clofed during the night, as well as feveral of the common ones.

The 27th, I reftored thefe bees to liberty; Burnens examined the cells with the greateft attention, and found jelly in all thofe

which

which ftill contained worms, but moft of them were fhut with a lid of wax: he examined fome of the latter, and found the worms employed in fpinning cocoons of filk.

All the worms had therefore been tended as in the natural hives. In this fecond trial we did not perceive any diforder in this hive; there had not been the leaft agitation: it is true fome of the working bees attempted to go out in the courfe of the day, but finding it impoffible, they re-afcended the combs quietly, which were never left for an inftant. The hive being abundantly fupplied with honey, and with the pollen neceffary for their young, left them nothing to wifh for; and they were ftill more happy when a queen was born, who afterwards became pregnant, and laid a great number of eggs.

After thefe two experiments there could be no more doubt that the fecundating duft was the aliment proper for the young bees, and that the want of this fubftance was the caufe of their death, and of the evident anguifh of their nurfes during their firft captivity.

The pollen is therefore the food of young bees.

FOURTH EXPERIMENT.

On Bees deprived of Honey and Pollen, and which it was attempted to feed with Sugar.

I wifhed to know if it was the faccharine part of the honey which enabled the bees to produce wax.

Burnens confined a fwarm in a glazed hive: one pound of Canary fugar was their fole aliment.

Exp. IV. What part of the honey contains the principles of the wax?

He put a fecond fwarm into another hive, and endeavoured to feed them with very coarfe raw fugar; and to obtain a term of comparifon, a third fwarm was fhut up in the fame manner, and fed with honey.

The bees of the three hives produced wax; thofe fed with the different qualities of fugar produced it fooner than the fwarm which had only had honey, and they produced it in greater quantity.

A pound of Canary fugar reduced to fyrup, and clarified with white of egg, yielded 10 gros, 52 grains, of a wax not fo white as that which the bees extract from honey.

Wax made from true fugars of different qualities.

An equal weight of raw fugar gave 22 gros of very white wax.

Maple

Maple fugar produced the fame effect. This experiment having been repeated feven times fucceffively, always employing the fame bees, we could not doubt that fugar contains the principles of wax, and we concluded that it was the faccharine part of the honey which had this property.

CONCLUSION.

Conclufions:

Thefe obfervations fhew,

1ft, That the wax comes from the honey :

2d, That the honey is alfo a food of the firft neceffity to the bees :

3d, That flowers do not always contain honey, as has been imagined; that this fecretion is fubject to the variations of the atmofphere; and, that the days in which it is abundant are very rare in our climate:

4th, That it is the faccharine part of the honey which enables the bees to produce wax:

5th, That raw fugar yields more wax than honey, or refined fugar:

6th, That the duft of the ftamina does not contain the principles of wax:

7th, That this duft is not the food of the adult bees, and that they do not collect it for themfelves:

8th, That the pollen affords the only aliment which is proper for their young, but that this fubftance muft undergo a peculiar elaboration in the ftomachs of the bees to be converted into an aliment, which is always appropriated to their fex, their age, and their wants, fince the beft microfcopes do not fhew the particles of pollen, or their coverings in the liquor prepared by the working bees.

I fhall fpeak of the economical confequences of thefe obfervations on another occafion. By fhowing the breeders the real wants of the bees, they will be poffeffed of the means of affifting them in time, in all their neceffities, and of preferring them in climates in which nature has not placed them, and in which they could not profper without our aid.

XII.

An Enquiry concerning the Nature of Heat, and the Modes of its Communication. By Benjamin *Count of* Rumford, *V. P. R. S. &c. Abridged from the Philofophical Tranf-actions for* 1804.

(Concluded from p. 63.)

Exp. 12. TWO equal cylindrical veffels of fheet brafs po-lifhed very bright, each three inches in diameter and four inches long, fufpended by their oblique necks in a horizontal pofition, being placed on their wooden ftands, were filled with water at the temperature of 180°; and their circular flat bottoms were prefented, in a vertical pofition, to the two balls of the thermofcope, at the diftance of two inches.

Exp. 12. Bodies alike in all refpects and heated, do, at equal diftances, affect the thermofcope equally.

When the two hot bodies were prefented, at the fame moment, to the two balls of the inftrument, or what was ftill better, when two fcreens were placed before the two balls, at the diftance of about an inch, and, after the hot bodies were placed, thefe fcreens were both removed at the fame inftant, the bubble remained without motion in the middle of the horizontal part of the tube of the inftrument.

If the diftances of the equally hot bodies were rendered unequal, the bubble always moved towards the moft remote of the two; and if a fingle hot body was prefented to one of the balls, the bubble was driven from it, and might have been carried quite out of the tube; which, however, was always avoided, as the inftrument would have been by that means quite deranged.

Exp. 13. The flat circular bottom of one of the cylindrical veffels was blackened by holding it over the flame of a wax candle, the other veffel remaining bright as before. Both were then filled with water at 180°, and prefented at equal diftances to the two oppofite balls of the inftrument, as defcribed in the laft experiment.

Exp. 13. A cylinder blackened with fmoke, gives out more radiant heat than a clear metallic furface.

The bubble was inftantly driven out of its place by the fuperior action of the blackened furface; and it did not return to its former ftation until the blackened furface had been removed to more than eight inches from the ball to which it was

prefented; the other, which had not been blackened, remaining in its firft pofition, at the diftance of two inches.

Other fimilar experiments were made to fhew whether thofe coatings or clothings which, in the former experiments with the large veffels, had accelerated their cooling, did alfo increafe their power of radiation, as fhewn by the procefs and inftruments laft defcribed. And the refults invaiiably proved that this is the cafe.

It was alfo a queftion deferving to be inveftigated, whether any peculiarity belonging to the metallic furface hitherto ufed, namely of brafs, might have influenced the refults.

Exp. 14, 15. The two large cylindrical veffels No. 1 and 2, were covered with a fingle coating of oil varnifh, and on this, when fufficiently dry, was laid a covering of thick gold leaf upon No. 1, and thick filver leaf on No. 2. Thefe veffels were cooled through the interval of 10 degrees, in the fame time as the naked veffel ufed as the ftandard. Similar experiments with veffels of tinned iron and of lead, fhewed that the radiation from all thefe metals, though fo different in their conducting power, is the fame.

" Is not this," fays the Count, " owing to their being all equally wanting in tranfparency ? And does not this afford us a ftrong prefumption that heat is, in all cafes, excited and communicated by means of radiations or undulations, as I fhould rather choofe to call them ?"

And he proceeds to obferve, that another very important queftion alfo muft be decided before thefe points can be determined, and that is, Whether bodies are cooled in confequence of the rays they emit, or by thofe they receive ? The celebrated experiment of Pictet has fhewn, in our author's opinion, that rays or emanations *proceed from cold bodies*, which may be concentrated by concave mirrors, and will affect a delicate air thermometer.

Exp. 16, 17. The horizontal cylindrical veffels *(Fig.* 3, *Plate I.* of the prefent volume) were made very clean and bright, were duly fixed, and left for feveral hours in a room near the thermofcope; and when each veffel was in fucceffion prefented to that inftrument, with every precaution to prevent irregularity from external circumftances, it was not found to be affected by them. One of the veffels was now taken away and filled with ice and water, and then prefented, at the

diftance

diftance of four inches, to one of the balls of the thermofcope. The bubble immediately moved towards the cold body, and paffed through the fpace of one inch. A nearer approach of the cold body produced a ftill farther motion in the fame direction.

Exp. 18, 19. Though this refult appeared to the Count to prove indifputably that cold bodies emit rays capable of generating cold in warmer bodies, yet, from the importance of the fact, he chofe to vary the fubftance prefented to the inftrument, as well as to remove all fufpicion of the action of cold currents of air. He therefore laid the thermofcope on one fide, and placed underneath one of its balls a folid cake of ice, at the diftance of fix inches. The refult of this experiment was the fame as of the other, and the bubble was moved one inch in the tube. Ice-cold water produced the fame effect as ice itfelf.

Exp. 20. Whether this effect of cold bodies be governed by the fame laws as thofe obferved by varying the nature of the furface in heated bodies, or by any other, now remained to be afcertained. It was before found that metal blackened over a candle, did emit much more of calorific rays than the fame metal when naked. The fame experiment was now made with cold bodies. One of the cylinders had its end blackened, and the other cylinder was left bright and naked. Both were filled with ice and falt, and at the fame inftant they were fuffered to act from equal diftances on the thermofcope. The bubble moved towards the blackened body; not indeed fo much as when the bodies were heated in the former experiments, becaufe the temperature was not here fo far diftant from the common temperature of furrounding bodies as in that experiment; but on feveral repetitions of the experiment with thefe cold bodies, the effect was conftantly the fame. It was found that the precipitation of ice out of the furrounding air tended fpeedily to raife the temperature, and alfo that the clean furface, when covered with ice, had a greater frigorific effect than when the metal was naked.

Exp. 21. The radiation of heat from animal fubftances appearing, from fome facts, to be confiderable, a piece of goldbeater's fkin was applied wet to one of the veffels, and remained firmly adherent when dry. This, when filled with hot water at 180°, and prefented to the thermofcope in oppofition

to

to the other veffel clean and bright, and alfo filled with the fame fluid at the fame heat, was found to be much more calorific. The bubble which moved from the coated body, did not return to its ftation until this body was removed to a dif-tance five times as great as that of the other. Whence our author concludes, that it emitted twenty-five times the quan-tity of calorific rays.

Exp. 22. The veffels ufed in the laft experiment were emptied and refilled with ice and water. They were then prefented at equal diftances from the refpective balls; and the effect of the body which was covered with gold-beater's fkin, was much more confiderable in producing cold.

The radiation of cold bodies appearing to the Count thus to be proved beyond all doubt, he was defirous of afcertaining whether the frigorific rays poffefs an equal power of affecting the temperature of bodies as the calorific rays do; the tem-peratures of the radiant bodies being at the fame diftance each way from the body to be acted upon.

Exp. 23. With this intention, one of the veffels filled with pounded ice and water, was prefented to a ball of the inftru-ment, and the other veffel filled with water at 112°, was prefented at an equal diftance on the oppofite fide of the fame ball; the temperature of the room and inftrument being 72°, or 40° diftant from each of the temperatures of the veffels, and the other ball of the inftrument being defended from all radiation by fcreens. The bubble remained motionlefs; fo that the oppofite actions were in fact equal. And when either veffel was drawn farther off, the effect of that veffel became lefs, and the bubble moved; that is to fay, towards the ball if the cold veffel were neareft, or from the ball if the hotter; and thefe effects were equal in quantity as well as in celerity of motion.

The Count again repeats his conclufion from thefe expe-riments lately exhibited to Profeffor Pictet, M. de Sauffure, and M. Senebier at Geneva, that the rays which generate cold are juft as real and juft as intenfe as thofe which generate heat; and he proceeds to account for this refult having been overlooked, by obferving, that the degrees of cold we are able to produce, are much lefs diftant from the ufual temperature than thofe of heat, which are within our power. Thus a can-non ball, heated to 160 degrees, or 70 degrees above blood-heat,

heat, would radiate quite as much as ice; and a bullet of freezing mercury would radiate fcarcely more than another of boiling water: both which hot temperatures are very trifling in comparifon to the heat in which we are habituated to notice and obferve this phenomenon.

Exp. 24. After the proof that cold bodies of the fame kind affect the thermofcope equally, when equally diftant from the common temperature, it remained to be determined whether the different modifications of furface have the fame effects in the ready propagation of lower temperatures, as they had fhewn before in higher. To fhew this, it was only required to oppofe them to each other, as to their action upon the fame ball, as in the laft experiment. With this view both difcs were blackened, and, the temperature of the room being 72° as before, one of them was charged with ice and water, and the other with water at 112°. Thefe, at equal diftances from the ball as before, did not affect the bubble, and therefore their actions were precifely equal.

Exp. 24. The fame furfaces which produce , moft heat by ra-diation, do alfo produce moft cold.

In the confideration of two kinds of rays, calorific and frigorific, it did not efcape the attention of our author, that hot and cold are terms denoting mere relations; fo that the fame body will be either hot or cold accordingly as the common temperature, or temperature of the bodies of compari-fon, is lower or higher. Queftioning, therefore, as to the difference between calorific and frigorific rays, he demands whether the fame rays may not be either calorific or frigorific, accordingly as the body at whofe furface they arrive, is hotter or colder than that from which they proceed?

Do not the fame rays produce heat or cold, as the temperature of the emanating bodies is higher or lower than the receiving body,

Exp. 25. The whole of the external furface of one of the large cylindrical paffage thermometers, *Fig.* 1, *Plate I.* was covered with gold-beater's fkin, and, along with the ftandard inftrument of the fame kind, was filled with hot water. The covered inftrument cooled through the ftandard interval of ten degrees, namely, from 101¼ to 91¼, in twenty-feven and three quarter minutes; but the naked inftrument employed 45 minutes in paffing through the fame interval.

Exp. 25. Metal covered with gold-beater's fkin cools fafter.

Exp. 26. Both inftruments were fuffered to remain in the cold all night, when the temperature in the naked inftrument was 50¼°, and that of the covered inftrument 49¼°; the air of the room being 48°. Both inftruments were then removed into a warm room, of which the temperature continued be-

Exp. 26. And alfo acquires heat more quickly.

tween

tween 64° and 65°. The covered inftrument acquired heat the moft rapidly; for in a quarter of an hour both ftood at 51½°, and at the end of four hours the naked inftrument fhewed the temperature 61°, and the covered 63½° : whence the Count obferves, that thofe fubftances which part with heat the moft readily, have alfo the greateft facility to acquire it.

He then proceeds to reafon on the probability that the temperatures of bodies may be changed, not only by the rays they emit, but by thofe they receive from other bodies ; and as the cooling of hot bodies is fo much accelerated by covering their furfaces with fubftances which affect the radiation or abforption, he thinks it highly probable that the air is but little affected as to its temperature, by thefe rays which pafs through it; and he contemplates the eftablifhment of this fuppofition, as promifing to explain various interefting phenomena; particularly that of the fteady temperature of living animals, notwithftanding the great quantity of heat generated in their lungs, and the different temperatures of the fluid which furrounds them.

For it is evident, the greater power an animal may poffefs of throwing off heat by radiation, independently of the effect of the contact of the furrounding air, the lefs will his temperature be affected by the changes in that fluid, or the oppreffive heat of the climate. The probability that negroes and people of colour, who fupport the heats of tropical climates much better than white people, offers itfelf in this place as the confequence of their colour; a quality which not only enables them to throw off heat, but even, as the Count much fufpects, to abforb frigorific rays from fuch bodies as may emit them.

Exp. 27. When the flat ends of both the horizontal cylindrical veffels were covered with gold-beater's fkin, and one of them painted black upon the covering with Indian ink, this laft, as indicated by the thermofcope, emitted more calorific rays from the included hot water than the other veffel did.

Exp. 28. When the fame two veffels filled with boiling-hot water, were fet to cool, the blackened veffel cooled through the ftandard interval of ten degrees in 23½ minutes, while the other, which was not blackened, employed 28 minutes.

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The author again reverts to the application of thefe facts to animal bodies. Whether the oily coating which favages apply to their fkins in cold climates, may not add to their comfort by reflecting frigorific rays; whether the Hottentots, ftill more difguftingly befmeared, may not derive advantages fimilar to thofe derived by negroes from their black colour? are queftions that promife to lead to refults of practical value. He then proceeds to explain more fully the manner in which negroes may be fuppofed to refift the action of a burning fun. An oil exudes from the fkin of thefe people when expofed naked to the fun; which oil reflects the fun's calorific rays. Heat more intenfe produces fweat, which not only aids the former procefs, but generates cold by evaporation. But when the fun is fet, the oil retires from the furface, and the fkin becomes well adapted to admit frigorific rays from the neighbouring bodies.

Exp. 29. The thermofcope was perceptibly affected by the radiation of cold bodies: It was defirable to know whether this effect would be fhewn in a groffer way, by accelerating the cooling of a hot body. For this purpofe, two conical veffels of thin fheet brafs, each four inches diameter at the bafe, and four inches high, ending above in a cylindrical neck, were feparately enclofed in a cylinder of thin pafteboard covered with gilt paper, and then the veffels were covered up with rabbit-fkins having the hair on them, in fuch a manner that no part of thefe veffels, except their flat bottoms, was expofed naked to the air. The bottoms were covered with gold-beater's fkin painted black with Indian ink, in order to render them as fenfible as poffible to calorific and frigorific rays.

The two veffels thus prepared were fufpended, with their bottoms downwards from the arms of a ftand, and under each was placed a pewter platter blackened on the infide by fmoke from a candle. The platters themfelves were fupported on fhallow earthen difhes which refted upon wooden ftands; each pewter platter having a perforated cover of thick paper, in the center of which was a hole fix inches in diameter. The diftance from the floor of the room to the fmoked furface of each platter was 40 inches, and the interval between the furface of each conical veffel and its correfpondent platter immediately beneath, was four inches. One of the platters remained at the temperature

ture of the room, but the other was kept conftantly ice-cold by means of pounded ice and water contained in the difh beneath it.

The two conical veffels were now filled with boiling hot water, and every precaution was taken to prevent agitation in the air; the only means by which an afcent of cold fluid could be fufpected. The refult of the experiment was, that the veffel fufpended over the ice-cold platter cooled from 50° to that of 40° above the temperature of the room, in 33 minutes and 42 feconds; whereas the other veffel required 39 minutes and 15 feconds to cool through the fame interval.

Exp. 30. Upon repeating this experiment the next day, the times were 33 minutes 15 feconds and 39 minutes 30 feconds.

As the cooling of thefe veffels is a complicated procefs which includes the confideration of the heat that paffed through the covered fides, in thefe different times; the Count enters into a procefs of computation, founded on the principles made ufe of at page 61, by which he determines that the velocity with which the heat paffed through the bottom of the veffel expofed to the ice, was to that with which it paffed through the bottom of the other veffel as five to four nearly.

Exp. 31. By the manner of defending the two conical veffels each had a circular band or hoop of the fine poft paper which projected half an inch below its bafe. It is evident that the air in this fpace could not pafs upwards, as it became heated by contact, and confequently, that very little of the cooling effect could have been produced by the contact of an afcending current. To afcertain what this effect might have been, the two veffels were fufpended as before; but one of them had its bafe inclined in an angle of 45°, while the other bafe continued horizontal. In this fituation they were filled with boiling water, and fuffered to cool without the platter and ftands beneath them. Two effects would follow from thefe arrangements: the veffel, which in the other experiment had been placed over a platter at the temperature of the room, would now cool a very little fafter by the abfence of that platter, which no doubt muft have had its temperature a little raifed; and the inclined veffel in the prefent experiment would be cooled fomewhat more fpeedily, by the fucceffive contact of the afcending current of air which was at liberty to rife. This veffel was found to cool through the ftandard interval of 10° in 37¼ minutes, and the

horizonta

Exp. 30. Repetition.

Velocity of cooling accelerated by the radiation from ice, in the ratio of 5 to 4.

Exp. 31. Procefs to fhew the heat abforbed by the air when a current was allowed to take place. It was then one twentyfeventh of the whole lofs.

horizontal veffel employed 38½ minutes. By referring thefe facts to computation, the Count finds that the heat loft by actual communication to the air, is nearly $\frac{1}{27}$ part of the whole lofs.

Exp. 32. The counter radiation by the platter, which was ftated under the laft experiment as impeding the cooling procefs, affords the important profpect of explaining the effect of clothing, and therefore deferved to be more fully examined. The experiments 29 and 30 were therefore repeated, with the diftance of three inches only between the bottom of each veffel and its correfponding platter. The times of cooling through 10°, were now 33¼ minutes and 40¼ minutes.

Exp. 33. And when the diftance was diminifhed to two inches, the times proved 32½ minutes and 42¼ minutes.

Thefe experiments fhew that the vicinity of a cold body, of which the low temperature is not kept up by artificial means, retards the cooling of a hot body. And from this fact the Count concludes, that if the hot body had been a globe fufpended in the center of another larger thin hollow fphere, of the fame temperature, at the commencement, as the air and walls of the room, the cooling would have been more flow than if the external globe had not been prefent; and alfo, if the external globe itfelf were included in another globe of the fame defcription, the retardation would have been ftill more confiderable. And by extending this fuppofed experiment to a number of thin concentric hollow fpheres, we may conceive a great retardation to follow, and fhall become acquainted with the nature of the effects which take place when a hot body is furrounded by proper clothing.

If the fpheres were metallic, the cooling would be flower if the furfaces were polifhed than if unpolifhed or blackened; whence it is highly probable, that the warmth of any clothing depends very much upon the polifh of its furface.

The microfcope fhews that thofe fubftances which fupply us with the warmeft coverings, fuch as furs, feathers, filks, and the like, have their furfaces highly polifhed; and the finer the fibres, or the greater number of interpofed polifhed furfaces, the warmer is the clothing.

In the former experiments of Count Rumford, he confidered the warmth of clothing as principally depending on the obftacles it oppofes to the motion of the furrounding cold air;

but, by a patient and careful examination of the subject, he is convinced that the efficacy of radiation is much greater than he had suppofed: from the refult of the experiment No. 31, it appears that a very fmall part of the heat of a body cooled in the air, is in fact communicated to that fluid; much the greateft portion being communicated to furrounding bodies at a diftance; and, in one of his former experiments, a hot body was cooled, though it was placed in a torricellian vacuum.

Air heated only at its furface and by radiation.

He confiders the heat which air receives by coming in contact with a hot body, to be communicated by radiation, in the fame manner as it is received by other bodies at a greater diftance; and he apprehends that the contiguous particle receives the heat in preference for no other reafon than becaufe it is at the furface of the fluid, this being the place where reflection, refraction, and increafe of temperature, take place; and, from thefe confiderations, he explains what has been called the non-conducting power of tranfparent fluids.

Analogy between light and heat.

By extending the analogy of thofe facts which we know concerning the effect of polifhed furfaces on light, to the radiations of heat alfo, the preceding facts are eafily explained. The frigorific rays are reflected externally, and a large portion of the calorific rays, which would have iffued forth through a rough furface, are, in the other cafe, turned inwards by reflection.

A drop of water refifts the heat of ignited iron, by its polifh.

The polifhed furface of a drop of water, which rolls about at a diftance from the face of a piece of red-hot iron, enables it to reflect the calorific rays; the water acquires little heat, and evaporates flowly.

With a lefs heat the water enters the pores of the oxide upon the metal, lofes its polifh, acquires heat very rapidly, and is foon evaporated.

It retains its polifh longer in filver, and refifts more.

If the metal be lefs oxidable, as, for example, a filver-fpoon, the drop of water will fupport or refift a lower heat. In fact it does not fo foon lofe its polifh; but at a ftill lower heat, that is to fay, a little above boiling-water, a drop of water is inftantaneoufly evaporated.

Exp. 34. Water rolling in a fpoon blackened with fmoke, cannot be made to boil.

Exp. 34. A clean polifhed fpoon, rendered black by holding it over the flame of a wax candle, will receive a large drop of water, which will roll about without wetting the blackened furface. This drop cannot be made to boil by holding the fpoon over the flame of a candle. When the

fpoon

fpoon is too hot to be held by its handle, the drop of water poured into the palm of the hand is warm, but by no means fcalding hot.

Exp. 35. If a large drop of water be formed at the end of a fmall fplinter of light wood, and the drop be thruſt quickly into the center of the flame of a newly fnuffed candle, it will remain for a confiderable time in the center of the flame, without being apparently affected by the heat; and if it be taken out of the flame and put upon the hand, it will not be found to be fcalding hot. If it be held for fome time in the flame, it will be gradually diminiſhed by evaporation; but it does not appear that the heat is communicated by the flame, but by the wood to which it adheres, which is foon heated, and at laſt fet on fire.

The remainder of the Count's memoir confifts of theoretical remarks and inferences, occupying 29 pages of the Tranfactions. I have endeavoured faithfully to defcribe the facts in the way of abridgement, but cannot with the fame facility do juftice to thefe argumentative refults. I fhall therefore, for the prefent, conclude my account of his paper; but may not perhaps wholly overlook his theories upon fome future occafion.

XIII.

Letter from Profeſſor VEAU-DE-LAUNAY *to* J. C. DELA-METHERIE, *on fulminating Silver* *.

AS it is at all times ufeful to ftate facts, whatever may be the refults, I think it right to inform you of an accident which occurred in my laboratory.

I had employed one of my pupils, a very good operator, to prepare a fmall quantity of fulminating filver, which he executed with ſkill.

The quantity obtained was about five grains, or a quarter of a gramme: it was depofited in a cryftal capfule about two lines in thicknefs. He had taken a fmall quantity, about half a grain, which was feparated with a card, and then dried, and

* From Journal de Phyfique, &c. Floreal, An. XII.

afterwards

' afterwards detonated by flight friction. Next day, that is to fay twenty-four hours after the preparation, this young perfon was defirous of taking an equal quantity from the capfule to repeat the experiment, but he had fcarcely touched the preparation with the corner of a card when a violent detonation and explofion took place in the capfule which was fhattered into a thoufand pieces. His face was covered with the vaporifed preparation, which was almoft black, and adhered ftrongly to the fkin: his eyes experienced a ftrong fhock, which produced extreme pain ; the opaque cornea became red and inflamed. Happily his fear was the greateft evil : by wafhing and bathing his eyes and face frequently with cold water, the effects of the detonation were foon diffipated.

Fortunately, none of the fragments of the glafs had touched his eyes or his face ; they were thrown nearly in a horizontal direction, to a confiderable diftance: Some were thrown upwards of twelve feet.

As the effects of this preparation may have more calamitous confequences. I think it ufeful to be guarded againft the dangers which it may occafion.

XIV.

Pyrotechnic Obfervations, with their Application to evaporating Furnaces*. By Cit. CURAUDAU, Corrcfponding Member of the Apothecaries Society of Paris, and Refident Affociate at the Athenæum of Arts.

SINCE I publifhed my obfervations on the caufes of the imperfections of evaporating furnaces, I have had occafion to make others on the fame fubject, the application of which may add to the advantages in their conftruction which I have fiade known.

Difadvantages in the conftruction of evaporating furnaces, &c. — In my firft memoir, I proved that the bottom of the copper in the evaporating furnaces, not only obftructs the elevation of the temperature, but alfo diminifhes the activity of the fire, and is rather favourable to the gazification of the combuftible body, than to its oxigenation: I cited the lamp of Argand with its

* From Annales de Chimie, No. 149, Floreal, An. XII.

glafs

glafs chimney, as an example of the neceffity of raifing the
température round the combuftible, whenever a compleat and
powerful combuftion is required. At prefent, I fhall take the
enameller's lamp as an example in fupport of my new obferva-
tions : I may fay that I am indebted to the examination of its
effects, for thofe which it has led me to make. In fact, if the Effects of the
jet of the flame of an enameller's lamp be examined, it will be current of air
on the flame of
found that the intenfity of its action depends on the current of Argand's lamp,
air which is directed on the flame of the wick ; it will alfo be
feen, that it is only at the extremity of its jet that the greateft
energy of the calorific rays exifts, and that its intenfity is fuch
that, by means of this lamp, effects may be produced, which
are, comparatively, as powerful as thofe obtained in our beft
furnaces.

This mode of action of the caloric proves, therefore, that its
effects may be augmented, by augmenting the rapadity of its
current, and by directing it fkilfully upon the body to be heated.
Thefe are the conditions which I have endeavoured to unite in
my new conftruction, and which, agreeably to the application
I have made of them, are employed to fupport an opinion which
required an experiment on a large fcale to efcape being placed
in the clafs of hypothefes.

Having been lately confulted upon the conftruction of a applied to works
brewer's furnace, I took the opportunity to fhow the great ad- on a large fcale.
vantages of the alterations which my obfervations appeared to
me to render neceffary, and to induce the proprietor to conftruct
his furnace according to the plan I fent him.

The following is the refult of the experiments which were Comparifon of
made to afcertain the advantages poffeffed by the new furnace two furnaces.
over that which it replaced.

The old furnace required 2¾ hours to raife the temperature
of 2500 litres of well-water to 50° of Reaumur, and confumed
in the operation, which was repeated daily, 625 kilogrammes
of new dry wood.

The prefent furnace, on the contrary, confumes only 450
kilogrammes of wood in the fame operation, and is only one
hour in raifing the temperature of 2500 litres of well-water
to 50° : Whence it refults, that this new conftruction evidently
makes a faving of $\frac{7}{4}$ in time and nearly one third in the com-
buftible.

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Such advantages appeared to me to be of fufficient importance to deferve to be known, and to make it defirable that advantage may be taken of a new procefs which will have great influence on the economy of the fuel neceffary in manufactures.

Defcription of the Furnace. Plate

A. Opening of the fire-place: it is 16 inches wide and 13 high.

B. This part of the furnace refembles a vault: It is $21\frac{1}{2}$ inches in height, $31\frac{1}{2}$ in width, and 5 feet in depth. To add to the effect produced by the heat, I give the middle of the fire place a depth of 2 inches more that at its fides, which makes it preceptibly concave.

C. An opening made in the middle of the vault, and which is intended to increafe the rapidity and the action of the calorific rays; it is 5 inches high, and is equal in thicknefs to the vault. In its lower part, this opening is $23\frac{1}{2}$ inches long, and $17\frac{1}{2}$ wide; and, in its upper part, is $19\frac{1}{2}$ long, and $13\frac{1}{2}$ wide, which gives each extremity of the opening the form of a fpheroid whofe longeft axis is in the direction of the length of the vault.

D. The diftance of the copper from the orifice for the heat is 5 inches in the middle, and is reduced to 4, at the angle E, which gives the advantage of concentrating the calorific rays, in proportion as they lofe their intenfity by their expanfion.

From the angle E to that at F, there is a diftance of 15 inches: the re-entering angle of F is $2\frac{1}{4}$ inches from the copper, and its faliant angle only one inch.

G and H are angles fimilar to that at F, but which are 11 inches diftant from each other: thefe angles may be multiplied according to the height of the copper. The advantages which they give confift in making the calorific ftream undergo feveral breaks, which increafes their power at the place of their deviation.

I is an opening communicating with the chimney: it is 27 inches wide and 5 inches high. At the angle H, half the circumference of the furnace fhould be clofed by a row of bricks, for the purpofe of forcing the heat to direct itfelf from
the

the side oppofite to the chimney; immediately above this courfe of bricks, thofe which fucceed muft be removed four inches from the copper, and continue fo to the height of four inches; afterwards each courfe of bricks muft be brought nearer, fo that this refervoir of heat may be clofed at the height of five inches, which muft be done round all the circumference of the copper.

K is the opening of the chimney: it muft be three decimetres fquare through all its height, and at leaft four metres high.

L is a blower of hammered iron; it is one metre above the copper, and ferves to open or clofe the chimney at pleafure.

The proportions laid down in this plan are intended for a copper four feet three inches wide in its lower part, and forty inches deep.

XV.

An Account of a curious Phenomenon obferved on the Glaciers of Chamouny; together with fome occafional Obfervations concerning the Propagation of Heat in Fluids. By BENJAMIN *Count of* RUMFORD, *V. P. R. S. Foreign Affociate of the National Inftitute of France, &c. &c.* [*]

IN an excurfion which I made the laft fummer, in the month of Auguft, to the Glaciers of Chamouny, in company with Profeffor Pictet of Geneva, I had an opportunity of obferving, on what is called the Sea of Ice, *(Mer de Glace,)* a phenomenon very common, as I was told, in thofe high and cold regions, but which was perfectly new to me, and engaged all my attention. At the furface of a folid mafs of ice, of vaft thicknefs and extent, we difcovered a pit, perfectly cylindrical, about feven inches in diameter, and more than four feet deep; quite full of water. On examining it on the infide, with a pole, I found that its fides were polifhed; and that its bottom was hemifpherical, and well defined.

Cylindrical pit (in a mafs or fea of ice) containing water.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the fouth, as it de-

[*] Phil. Tranf. 1804, p. 23.

fcended;

scended; and, in consequence of this inclination, its mouth or opening, at the surface of the ice, was not circular, but elliptical.

They are frequently found.

From our guides I learnt, that these cylindrical holes are frequently found on the level parts of the ice; that they are formed during the summer, increasing gradually in depth, as long as the hot weather continues; but that they are frozen up, and disappear, on the return of winter.

Inference against the conducting power of water.

I would ask those who maintain that water is a conductor of heat, how these pits are formed? On a supposition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may easily be explained; but it appears to me to be inexplicable on any other supposition.

The quiescent mass of water, by which the pit remains constantly filled, must necessarily be at the temperature of freezing; for it is surrounded on every side by ice: but the pit goes on to increase in depth, during the whole summer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the *bottom* of the pit only, and not on its sides?

Solution of the effect by the greater density of water above zero.

These curious phenomena may, I think, be explained in the following manner : The warm winds which, in summer, blow over the surface of this column of ice-cold water, must undoubtedly communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before, by this small increase of temperature, sink slowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

This operation is exactly similar to that which took place in one of my experiments, (See my Essay on the Propagation of heat in Fluids, *Experiment* 17,) the results of which, no person, to my knowledge, has yet explained.

Constant temperature of water at the bottom of deep lakes, ffered in support of the non-

There is another very curious natural phenomenon, which I could wish to see explained in a satisfactory manner, by those who still refuse their assent to the opinions I have been led to adopt, respecting the manner in which heat is propagated

in

in fluids. The water at the bottoms of all deep lakes is con-
stantly at the same temperature, (that of 41° Fahrenheit,)
summer and winter, without any sensible variation. This
fact alone appears to me to be quite sufficient to prove, that if
there be any immediate communication of heat between neigh-
bouring particles or molecules of water, *de proche en proche*, or
from one of them to the other, that communication must be so
extremely slow, that we may with safety consider it as having
no existence; and it is with this limitation that I beg to be
understood, when I speak of fluids as being non-conductors of
heat.

In treating of the propagation of heat in fluids, I have hitherto
confined myself to the investigation of the simple matter of fact,
without venturing to offer any conjectures relative to the causes
of the phenomena observed. But the results of recent experi-
ments on the calorific and frigorific radiations of hot and of cold
bodies, (an account of which I shall have the honour of laying
before the Royal Society in a short time,) have given me some
new light respecting the nature of heat, and the mode of its com-
munication; and I have hopes of being able to show *why* all
changes of temperature, in *transparent* liquids, must necessarily
take place at their surfaces.

I have seen with real pleasure, that several ingenious gen-
tlemen, in London, and in Edinburgh, have undertaken the
investigation of the phenomena of the propagation of heat in
fluids; and that they have made a number of new and inge-
nious experiments, with a view to the farther elucidation of that
most interesting subject. If I have hitherto abstained from
taking public notice of their observations on the opinion I have
advanced on that subject, in my different publications, it was
not from any want of respect for those gentlemen that I re-
mained silent, but because I still found it to be quite impossible
to explain the results of my own experiments, on any other
principles than those which, on the most mature and dispas-
sionate deliberation, I had been induced to adopt; and because
my own experiments appeared to me to be quite as conclusive
(to say no more of them) as those which were opposed to them;
and, lastly, because I considered the principal point in dispute,
relative to the passage of heat in fluids, as being so clearly
established by the circumstances attending several great opera-
tions of nature, that this evidence did not appear to me to be in

danger of being invalidated by conclusions drawn from partial and imperfect experiments, and particularly from such as are allowed on all hands to be extremely delicate.

Heat of the sides of the vessel, and descending current in a vessel of ice.

In all our attempts to cause heat to descend in liquids, the heat unavoidably communicated to the sides of the containing vessel, must occasion great uncertainty with respect to the results of the experiment; and, when that vessel is constructed of ice, the flowing down of the water resulting from the thawing of that ice, will cause motions in the liquid, and consequently inaccuracies of still greater moment, as I have found from my own experience; and, when thermometers immersed in a liquid, at a small distance below its surface, acquire heat, in consequence of a hot body being applied to the surface of the liquid, that event is no decisive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche;* so far from being so, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is possible, for aught we know to the contrary, that it may be occasioned by the radiation of the hot body placed at the surface of the fluid.

Reference to the experiments of boiling water standing over ice.

In the experiments of which I have given an account, in my Essay on the Propagation of Heat in Fluids, great masses, many pounds in weight, of boiling hot water, were made to repose for a long time (three hours) on a cake of ice, without melting but a very small portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° Fahrenheit,) nearly twice as much ice was melted in the same time. In these experiments, the causes* of uncertainty above mentioned did not exist: and the results of them were certainly most striking.

The conclusions which naturally flow from those results, have always appeared to me to be so perfectly evident and indisputable, as to stand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards, to the cake of ice, on which it reposed, and from which it was separated only by a stratum of cold water, half an inch in thickness?

I wish

I wish that gentlemen who refuse their assent to the opinions I have advanced respecting the causes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewise wish that they would inform us how it happens, that the water at the bottoms of all deep lakes remains constantly at the same temperature: and, above all, how the cylindrical pits, above described, are formed in the immense masses of solid and compact ice which compose the Glaciers of Chamouny!

A remark, which surprised me not a little, has been made by a gentleman of Edinburgh, (Dr. Thompson,) on the experiments I contrived, to render visible the currents into which liquids are thrown on a sudden application of heat, or of cold. He conceives, that the motions observed in my experiments, among the small pieces of amber which were suspended in a weak solution of potash in water, were no proof of currents existing in that liquid; as they might, in his opinion, have been occasioned by a change of specific gravity in the amber, or by air attached to it. I am sorry that so mean an opinion of my accuracy as an observer should have been entertained, as to imagine that I could have been so easily deceived. For nothing surely is easier, than to distinguish the motion of a solid suspended in a liquid of the same specific gravity, which is carried along by a current in the liquid, from that of a body which descends, or ascends, in the liquid, in consequence of its relative weight, or levity. In the one case, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quiescent fluid, by the action of gravity, or rises, in consequence of its being specifically lighter than the fluid, it must necessarily move in a vertical direction.

The fact is, that I very often observed, in the course of my numerous experiments, the motions of small particles of matter, of different kinds, in water, which Dr. Thompson describes; but, so far from inferring *from them* the existence of currents in that fluid, their cause was so perfectly evident, that I did not even think it necessary to make any mention of them.

I cannot conclude this Paper, without requesting that the Royal Society would excuse the liberty I have taken in troubling

them

them with these remarks. Very defirous of avoiding every species of altercation, I have hitherto cautioufly abftained from engaging in literary difputes; and I fhall moft certainly endeavour to avoid them in future.

I am refponfible to the public for the accuracy of the accounts which I have publifhed of my experiments; but it cannot reafonably be expected, that I fhould anfwer all the objections that may be made to the conclufions which I have drawn from them. It will however, at all times, afford me real fatisfaction to fee my opinions examined, and my miftakes corrected; for my firft and moft earneft wifh is, to contribute to the advancement of ufeful knowledge.

XVI.

Account of two Sketches ; viz. one for a perpetual Motion, and the other of a jointed Parallel Rule, which has no fide Deviation. In a Letter from R. B.

To Mr. NICHOLSON.

SIR,

Perpetual motions defcribed in the Philofophical Journal.

I was much gratified feveral years ago by fome eſſays with which you obliged your readers upon the perpetual motion. In the firft volume, p. 375, of your quarto feries, I find an account of feveral fchemes (neceſſarily abortive) for producing perpetual motions by the action of gravity, and in your third volume, I find an account of various methods of keeping up the motion of a machine by means of the changes which take place in the barometer and thermometer. I have ventured to fend you the fketch of a project for a perpetual motion of the latter kind, which has long remained among my memorandums. You will fee that it is not of the clafs of perpetual motions properly fpeaking, but merely the application of fome exifting intermittent motions in nature to the purpofe of maintaining the rotation of machinery. If you fhould hefitate about inferting this, I think the other fketch which accompanies it of an ufeful inftrument, cannot fail to meet your approbation.

Defcription of a wheel kept in motion by the marine barometer.

Fig. 1, *Plate XII.* A reprefents the marine barometer of Halley, but varied by the addition of a veffel B at the open end, in which the water or other fluid, expofes a furface nearly

equal

equal to that in the clofed veffel A. Thefe two veffels are connected by a long horizontal tube G. It is evident that any change, either in the preffure of the external air or the elafticity of the internal air, will caufe the fluid to run along the tube, and add to the weight of A or of B according to circumftances. The heavier veffel will preponderate ; but it will be prevented from defcending too far by a ftop or bearing to which it will arrive. Any change in the inclination of G will move the attached lever C D ; by means of which, one of the two horizontal racks will be made to pufh round that ratchett wheel into which its teeth fall, at the fame time that the other rack will be drawn backwards upon its wheel. The oppofite action will drive forward the other wheel ; and as both thefe wheels are fixed on the fame axis, the fyftem will be driven the fame way by every change of denfity or weight in the air that takes place.

The other inftrument, *Fig.* 3, is a parallel rule which is framed without any fliding work, and opens to a much greater extent than ufual, without any fide deviation. I do not know the inventor; but it was communicated to me by a private hand. The jointed parts are reprefented in *Fig.* 2, and the dimenfions are expreffed by the fmall numerals. From the joint D to the joint E upon the bar, C E, the diftance, is = 1 ; from the joint E to the joint A, = 2 ; from the joint A to the joint B, = 3 ; and from the joint B to the joint D, = 4 ; which is alfo equal to the part D C of the Bar E C. Now it is found, and I muft leave the mathematical proof to your readers, that the point C moves by the opening from B (upon which it lies when fhut) in a ftrait line, very nearly at right angles with B A. From this property it is eafily feen, that two parallel rules, having the fyftem of four bars fixed to each, and connected as in the figure, will be moveable without fide deviation, and will preferve their parallelifm in all fituations.

Account of a new parallel rule.

I am Sir,

Your obliged humble fervant,

R. B.

Familiar

XVII.

Familiar Account of the Method of eftimating the Value of a Steam-Engine in Horfe-Powers as they are called. By a Correfpondent.

To Mr. NICHOLSON.

SIR,

Introductory letter.

AS your excellent Journal is the repofitory for ufeful information, whether fcientific or practical, I have thought I fhould oblige many manufacturers and others of your readers, by fending you a very clear report about horfe-powers, which a fiiend of mine has communicated to me, and was received by him from an eminent character in anfwer to an enquiry profeffionally made.

It clearly appears from this paper, that the calculation by horfe-powers muft be fallacious, unlefs engineers could agree as to the quantity of work they would arbitrarily, in the firft inftance, afcribe to one horfe; and then the expreffion would be nugatory. And not only fo, but it would not then be true that the value of a fteam-engine in work, however clearly expreffed in quantity *per* day, would be fairly fhewn, unlefs the wages or food of the working being were taken into the account. Coals may be ftiled the food of a fteam-engine, and nothing is more evident than that, if two engines raife equal quantities of water *per* hour, but confume different quantities of coal, they will not be equally beneficial to the proprietors. I would therefore propofe, that the eftimate fhould be made by attending to thefe two particulars only, and faying nothing about horfes, at leaft in fpecific arguments intended to have legal effects: And, as a fupplement to the facts and obfervations contained in the report, I will add, that one of the beft engines of Boulton and Watt, has been known to raife between 28 and 30 millions of pounds of water to the height of one foot with one bufhel of good coals, which appears to be an outfide meafure; and that, though there are fubfequent improvements both in the conftruction of furnaces and the working gear, yet there are fome amoug late engines which fall fhort of 20 millions.

I am, Sir,

Your conftant reader,

E. T.

Report concerning the Power of a Steam-Engine erected by Contract at * * * *.

IT is required to determine whether the steam-engine erected at * * * * by Mr. ——, be equal to the power of sixteen horses. The same has a cylinder of 21⅛ inches diameter, and gives 23 double strokes *per* minute, of four feet each.

In answer to this question it must be previously remarked, that steam-engines having originally been recommended and substituted instead of horses, the method of computing by the number of those animals intended to be supplied by means of this invention, has been generally applied, though it is much less certain and accurate than other methods well known to mechanical men. The uncertainty of calculating by horse-powers arises from various causes; such as the great differences of ability between the strong and heavy horses used in London, and those of not half the strength used in various parts of the country; the greater or less degree of speed during work; the quantity of re-action against which they are urged to pull; the shorter or longer time of work; their food, stabling, &c. &c. And this uncertainty, as may easily be conceived, is so great, that the words *horse-power* cannot practically be applied, otherwise than to denote a certain quantity of mechanic effect agreed upon and understood between engineers, and must not be understood to denote any elementary measure, capable of being worked out or inferred within any reasonable or useful limits, from the real power of the horse himself.

It therefore follows of necessity, that the engine must be examined by first stating its mechanic effect; that is to say, how many pounds weight it is capable of raising through a given space in a given time, that is to say, through the height of one foot during one minute, and then dividing this sum by the like effect producible by one horse, according to the statements and practice of engineers of the first reputation. I confine myself to Mess. Boulton and Watt, Dr. Desaguliers and Mr. Smeaton.

The practice of Mess. Boulton and Watt is, to consider a horse as capable of raising a certain weight, which is stated to be of 32000 pounds avoirdupois, one foot high in one minute. Desaguliers' results brought to the same form, give 27500 pounds;

Steam engine at * * * *.

Reason why steam engines have been compared with horses.

It is an uncertain method.

Words horse-power must denote an arbitrary quantity of effect.

Quantities of work per horse, by Boulton and Watt, Desaguliers and Smeaton.

pounds; and Smeaton's 22916 pounds, under the same circumſtances. The loweſt of theſe performances are more than equal to the average power of a horſe employed in huſbandry for eight hours *per* day.

A ſteam engine computed.

If the diameter of the cylinder be multiplied by itſelf ($21\frac{1}{8} \times 21\frac{1}{8}$), the product will give $446\frac{1}{4}$ round inches for the whole ſurface: and Smeaton reckons the effective or working preſſure *per* round inch on the atmoſpheric engine, at ſeven pounds avoirdupois. It is uſual to reckon the working preſſure on a cloſe engine (like that in queſtion), at 10 pounds the round inch; but I ſhall firſt take the ſeven pounds as being againſt the builder: So that, by multiplying the round inches $446\frac{1}{4}$ by 7, we have $3123\frac{3}{4}$ pounds for the weight raiſed. But the ſtrokes are 23 of four feet double *per* minute, that is to ſay, 184 feet. Multiply the weight $3123\frac{3}{4}$ by the height 184, and the product 574760 will be the mechanic effect of the engine, or the number of pounds it will raiſe one foot high in one minute. Laſtly, Divide this by Boulton and Watt's horſe-power ($\frac{574760}{33000}$), and the quotient $17\frac{1}{6}$, or very nearly 18, will expreſs the power of the engine in horſes.

If we follow Deſaguliers, the engine will prove equal to 21 horſes.

And, according to Smeaton, its power will be equal to 25 horſes.

If we had taken the preſſure at 10 pounds *per* round inch, the powers would have proved much greater, as below.

Steam engine can work treble days.

In the above calculations the horſes are ſuppoſed to be fairly worked, and the engine is ſuppoſed to be ſtopped as ſoon as the horſes leave off. But an engine can work the whole 24 hours; and Smeaton, conſidering that three ſetts of horſes muſt be kept to work conſtantly for the ſame time, reckoned a ſteam engine to be equivalent to three times as many horſes as could equal its rate of working. The following table will ſhew the powers of this engine, according to all theſe ſeveral methods:

7 lb.

	Power in Horfes ac-cording to Boulton and Watt.	Ditto ac cording Defagu-liers.	Ditto ac- cording to Smea-ton.	24 hours work according to Smeaton in horfes.
7lb. preffuture per round inch.	17 96/100 or 18 very nearly	21	25	75
10lb. preffuture per round inch.	25	30	35¼	107

XVIII.

Experiments proving the Neceffity of atmofpherical Oxigen in the Procefs of Vegetation. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

DR. Prieftley and M. Ingenhoufz have made many experi- ments with plants, confined in azote; from which philofophers have inferred, that vegetables differ from animals in not requir- ing the prefence and fupport of atmofpherical oxigen. One circumftance however occurs in all thefe experiments, if my memory be correct, which renders the conclufion doubtful: the plants, ufed in them were in contact with the water that confined the azote in the inverted jars, through the medium of which they had evidently the fame indirect communication with the atmofphere that fifhes are known to require.

Reflections on former experi-ments of the kind.

With a view to throw fome additional light upon this dif- putable point in the vegetable economy, I determined to fur- round certain fucculent plants entirely with azote; which pof- fefs the fingular property of vegetating when fufpended by the roots in dry air. My method of performing this confifts in fix- ing the fubject of the experiment by means of a thread, to a fmall perforated board; ftanding on a pillar, which was fur- nifhed with a foot of lead. The plant being thus fecured, was introduced through water into a glafs jar of azote; where it was wholly furrounded by the gas.

Experiments on fucculent vege-tables.

Exp. 1. The firft experiment, I made in this manner, was tried in June 1800. A plant of fedum acre was furrounded by azote when it was ready to expand its flowers, and another was placed befide it, under a fimilar jar; which contained

Experiment with fedum acre going to flower.

3   common

common air. The latter specimen flowered very well; but the former died away without opening one of its blossoms.

With the same in flower.
Exp. 2. The event was the same when I made use of a branch of this sedum already in flower; for the plant withered and did not shed its petals; which is contrary to the usual habits of this vegetable, when it is left to the impulses of nature unthwarted by art.

With barren plants of the same and sempervivum tectorum.
Exp. 3. Several plants of sedum acre were also confined in jars filled with azote last spring, before the flower buds were formed upon them: not one of these attempted to vegetate; on the contrary they all withered in the course of four or five weeks. This was not the case with other specimens, which were placed beside them upon similar stands, under jars containing common air and standing in water; for they remained green and vigorous, after the former were dead; they had moreover produced a number of purple filaments springing from the stalk; which is constantly done by the sedum acre, when suspended by the roots. It is proper to remark, that the air of the latter set was frequently changed, with a view to keep it near the standard of the atmosphere. The *sempervivum tectorum* did not offer to vegetate when treated in the same manner by being confined in azote; but it appeared to be more tenacious of life under this kind of treatment, than the sedum acre.

Inference from these experiments.
The preceding experiments induced me to conclude, at the time of making them, that the welfare of succulent plants requires them to be exposed to the atmosphere, and that they cannot grow when surrounded by azote, nor even preserve the vegetative principle in it, for an indefinite length of time.

Seeds and bulbs in the same predicament.
I have shewn in the fourth volume of the Manchester Memoirs, that seeds and bulbs are not less indebted for their prosperity to the agency of oxigen, than succulent plants appear to be from the present letter. Thus it is manifest that the farther we carry our researches into the nature of organized bodies, the greater is the number of them; which are found to be incapable of performing their natural functions, when deprived of respirable air; but as the truth of this maxim remains to be extended experimentally to vegetables of a less humid constitution, than the sedums, the following fact may be related with propriety.

Experiment made with cerastium vulgatum.
Exp. 4. A diminutive plant of *cerastium vulgatum* was placed on the 18th of February, 1804, upon a stand covered with a

2   slice

slice of wet sponge; it was then put under an inverted jar, containing common air, and standing in water. This plant encreased confiderably in bulk in the courfe of 20 days; befides which it had taken root in the fponge; it was therefore transferred into a jar filled with azote; but this change of circumftances put a ftop to vegetation; for the herb made no further advances, but withered away.

Exp. 5. In order to diverfify thefe experiments as much as poffible, I took a number of phials, having narrow necks, and filled them with water, which was either procured from fnow, or deprived of air by boiling. Proper plants being then placed in the bottles, they were introduced quickly through water into jars filled with azoe; under which circumftances, all my fpecimens died away fooner or later without making one effort to vegetate. The herbs ufed on thefe occafions were the common garden fpear-mint and the draco-cephalum Moldavicum; both of which vegetate freely in bottles of water expofed to the atmofphere. I alfo treated in the fame manner, a branch of Lyfimachia vulgaris, which was ready to flower; it declined gradually for a month, but never opened one bloffom; though it is an aquatic plant and flowers very well in jars of water. It may here be remarked once for all, that vegetables die very flowly in azote, provided they are fupplied with water deprived of air; but if my experiments may be relied on, all their natural funtions ceafe in thefe circumftances, as foon as they are removed from the atmofphere.

Exp. 6. The lofs of energy experienced by plants confined in mephitic air, does not appear to arife from an injurious quality of the gas, but from the want of the falutary ftimulus of oxigen; for a flip of fpear-mint, which had remained twelve days in a glafs of azote, recovered upon being reftored to the air, with the lofs of its loweft leaves only; and an off-fet of fempervivum vegetated freely after being removed from a jar of the fame gas, in which I had kept it, from the 2nd of April to the 2nd of May, ftanding in a bottle of fnow water. Thefe facts feem to argue, that vegetables, treated in the manner defcribed as above, fuffer a fpecies of torpidity, from which they may be recalled by the prefence of atmofpherical air, provided the re-admiffion of this neceffary agent be not delayed too long; in which cafe, the plant perifhes, in confequence of

the

Margin notes:
with plants in water deprived of air.

Plants die in azote for want of their proper ftimulus.

the continued inactivity of its organs, and the fermentation ot its juices; which is occafioned by the fufpenfion of all its functions.

Succulent plants abforb air from the atmofphere, but dry ones from water.

This fluid fo neceffary to the procefs of vegetation, is evidently abforbed from the atmofphere by fucculent plants, bulbs, and a variety of feeds, when properly moiftened; for they will all vegetate freely in open fituations; but the following experiment feems to prove that trees, fhrubs and the lefs juicy herbs derive air from water, as fifhes do.

Exp. 7. I introduced the lower extremities of different plants, or more properly of the branches of different plants, into phials filled with water, and after luting the necks with wax, I funk the bottles in pots of water, fo as to leave the foliage of the plants expofed to the fun and furrounded by the atmofphere. The evaporation caufed by the action of the light drew the water out of the bottles, and the air defcending at the fame time through the fubftance of the plants prevented a vacuum being formed in thefe clofe veffels. This fact had nearly perfuaded me, that vegetables poffefs a faculty analogous to refpiration; but when I repeated the experiment with phials perforated in the bottom, they remained full of water. This circumftance fhews the refpiration in queftion to be merely accidental and to be occafioned by the necks of the bottles being luted; on the other hand it is highly probable, that air enters the veffels of vegetables in conjunction with water. Here it is decompound, in confequence of the ftimulus given to the vegetative principle by its prefence; the azote is afterwards affimulated into the fubftance of the plant, while the oxigen is rejected either wholly or in part. This gas, after its expulfion flows into the mephitic air, contained in the jars, the volume of which is thereby augmented, and its quality improved; but I will not venture to fay, that the two gafes form a new chemical compound on thefe occafions.

Exception to the general rule that plants require air.

Exp. 8. I am acquainted with a fingle exception to the general rule that air is requifite in the procefs of vegetation. For when plants have withered in azote they become mouldy; that is, in the language of botany, they are covered with the mucor mucedo. The worms which inhabit the decayed livers of fheep, &c. furnifh us with a parallel inftance in the animal kingdom; and the inveftigation of thefe fingular anomalies forms a difficult problem in the natural hiftory of organized bodies.

The

The sentiments contained in this letter, are oppofite to the The effay concluded. common notions relative to the nature of vegetation, and fo manifeft a departure from eftablifhed opinions may be thought to require an apology. The juftification of my conduct however appears in the body of the effay. The experiments are related with an attention to circumftances; which will enable almoft any perfon to repeat them; and fhould fome philofopher undertake to do it, his labours will either eftablifh my opinions, or confirm the contrary doctrine, by the refutation of them. There is one omiffion in the detail of thefe experiments; I have neglected to remark, that the term azote fignifies nothing more than common air deprived of oxigen by the fulphuret of potafh or the martial pafte of fulphur, but more frequently by the former. The determination of the queftion is of fome moment, not only to natural hiftory, but perhaps to agriculture alfo; and I could wifh it to be refumed by a philofopher, whofe advantages are fuperior to my contracted powers of obfervation and imperfect apparatus. Should any man of fcience undertake the tafk, I would recommend the fedum acre to his notice; becaufe the filaments which fpring from the ftalk of this plant in common air afford a criterion whereby the progrefs of fimilar fpecimens confined in azote may be obferved and afcertained.

JOHN GOUGH.

Middlefhaw,
October 21, 1801.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Frothing of Oil by Electricity.

THE anonymous author of fome electrical experiments communicated to Van Mons the following fact, from whofe Journal I tranflate it. He was making an experiment to afcertain the conducting quality of oil, for which purpofe he had filled one third part of the length of a tube with the oil of Colfa. The tube was three feet long and half an inch in diameter, having its clofed end drawn out: The open extremity was fecured by a cork, through which a needle was thruft; and over the cork was a coating of wax to prevent the oil from tranfuding. The tube

tube being then placed on two infulating fupports, and the needle turned towards the part moft remote from the machine, the tube was flightly inclined, in order that the oil might run down to that part of its capacity which was neareft the prime conductor.   He then put the machine in action, and with great furprife obferved the oil gradually to enter into a ftate of ebullition; beginning at the furface and fpeedily acquiring a ftate of effervefcence perfectly refembling that of fparkling Champaigne.   Upon opening the tube, a gas efcaped with violence, which the author did not collect, but which well deferves to be examined.   This active electrician intends to fubject the different vegetable and mineral acids to the action of electricity by the fame procefs.

Native Magnefia.

GIOBERT has found that a white earth confidered as pure alumine and employed at Turin in the fabrication of porcelain, contains 0,80 of magnefia.   VAN MONS.

Gray's Experienced Millwright.

The Experienced Mill-wright; or a Treatife on the Conftruction of fome of the moft ufeful Machines, with the lateft Improvements. To which is prefixed a fhort Account of the general Principles of Mechanics and of the Mechanical Powers.   Illuftrated with forty Engravings.   By Andrew Gray, Millwright, very large quarto, 73 Pages of Letter Prefs, of the fame Size as the Engravings.

THE author is a practical mechanic, who has been upwards of forty years employed in erecting different kinds of machinery; and the greateft part of the Defcriptions in this Work are of Machines which he has either planned or fuperintended their conftruction.   Thefe Drawings being accurately made to fcale, are not, as he remarks, to be confidered as mechanical fpeculations, always doubtful till fubjected to actual experiment, but as cafes of practical knowledge, the effects of which have been fully tried and long approved ; moft of them being ftill employed for the purpofes of their original fabrication.

Previous to his Account of the Machines, this author gives a popular Treatife on Mechanics, in fix different chapters, beginning with General Principles and Definitions, which are followed

lowed by Explanations of Simple Engines, called the Mechanical Powers. Thefe are fucceeded by an Account of the Effects of Friction, and the beft Method of applying thofe Mechanical Powers. Practical Directions for defigning and conftructing the Parts of Machines are in the next place given ; and laftly, the author treats on the Strength of Machines, the relative Velocity of their Parts and the leading Doctrines which are neceffary to be attended to in the erecting of Water-Mills.

The Machines of which Meafured Plans, Elevations and Sections are given, are ; A confiderable number of Thrafhing Machines, to be drawn either by Water or by Horfes, with the Elevation of a Wind-mill to turn Machinery of this Defcription; one Snuff Mill; two Sheeling Mills; feveral Corn, Barley, Malt, and Flour Mills; one Oil Mill; one Flax Mill; one Paper Mill; one Saw Mill; one Fulling Mill; Bleaching Machines, Beetling Machines, and various Pumps.

Thefe Drawings and Defcriptions will no doubt prove of high value to the man of bufinefs; though it is perhaps to be regretted that the fame attention to junior ftudents which is difplayed in the introductory treatife, is not as fully fhewn by explaining the relative operations and effects of the parts of thefe machines in the defcriptions which accompany the drawings.

Practical Obfervations concerning Sea Bathing; to which are added, Remarks on the Ufe of the Warm Bath. By A. P. Buchan, M. D. of the Royal College of Phyficians. London. Crown 8vo. 207 Pages.

IN the early ftages of human fociety, when the occupa- tions of man led him to ftrong exercife in the open air, it is probable that his health may have been chiefly impaired by the fortuitous occurrences of his mode of life, and the direct wear of his organs engaged in the grofs and active labours of his life. It is probable that he may have felt little of thofe difeafes which the claffification and habits of a refined and luxurious fociety have introduced. Neither mental fatigue, nor the anxious and inceffant labour of crowded cities could have operated upon his powers, nor was he fubjected to the miferies of indolence, repletion, and the liftlefs want of motive which benumb the faculties of the rich. Thefe are the prices which

Buchan on fea bathing.

by

By our numerous miftakes we are compelled to pay for the fu-
perior enjoyments which an enlightened ftate of fociety can
afford to thofe only who poffefs the virtue, the conduct, and
the good fortune to know and to acquire them. Among the
moft oppreffive of thefe confequences, no doubt, is the lofs of
health; and while we make facrifices to what we imagine to
be our welfare, it becomes us occafionally to interrupt our
ufual train of habits, to unbend the mind, give repofe to the
fyftem, and recover, by an occafional change of our occupa-
tions, that vigour which famenefs of purfuits and confinement
feldom fail to impair.

One of the means at prefent much reforted to is, to repair
to the fea-coaft; partly with a view to enjoy a fcene which,
by its impreffive magnitude and the numerous changes it un-
dergoes, cannot but be highly interefting; but principally with
the intention of bathing in the fea. Bathing is certainly of the
greateft benefit in many cafes, and, like every other practice
capable of ftrongly affecting the human frame, it muft alfo, in
other-cafes be capable of operating to the injury of the health.
We have no direct general treatife on this fubject; and to give
the requifite information to individuals by which they may fafely
and judicioufly regulate themfelves requires fomewhat more
than mere medical fkill. The prefent work abounds with in-
terefting facts and clear deductions, and is written with that
candid fpirit of philofophical remark which fhews an intimate
acquaintance with the moral as well as the phyfical conditions
of our exiftence, and the accidents to which we are expofed.
I am much miftaken if any one who begins to read this work
will be difpofed to quit it without perufing the whole; and
there are few who can have fo extenfively confidered the fub-
ject, or who are fo little concerned in its difcuffion as not to
derive advantage from the many points of information it con-
tains.

Tallow Lamp by W. & H. Boswell.

Evaporating Furnace by Curaudau.

Mr. W. Bowe's double Brace &c
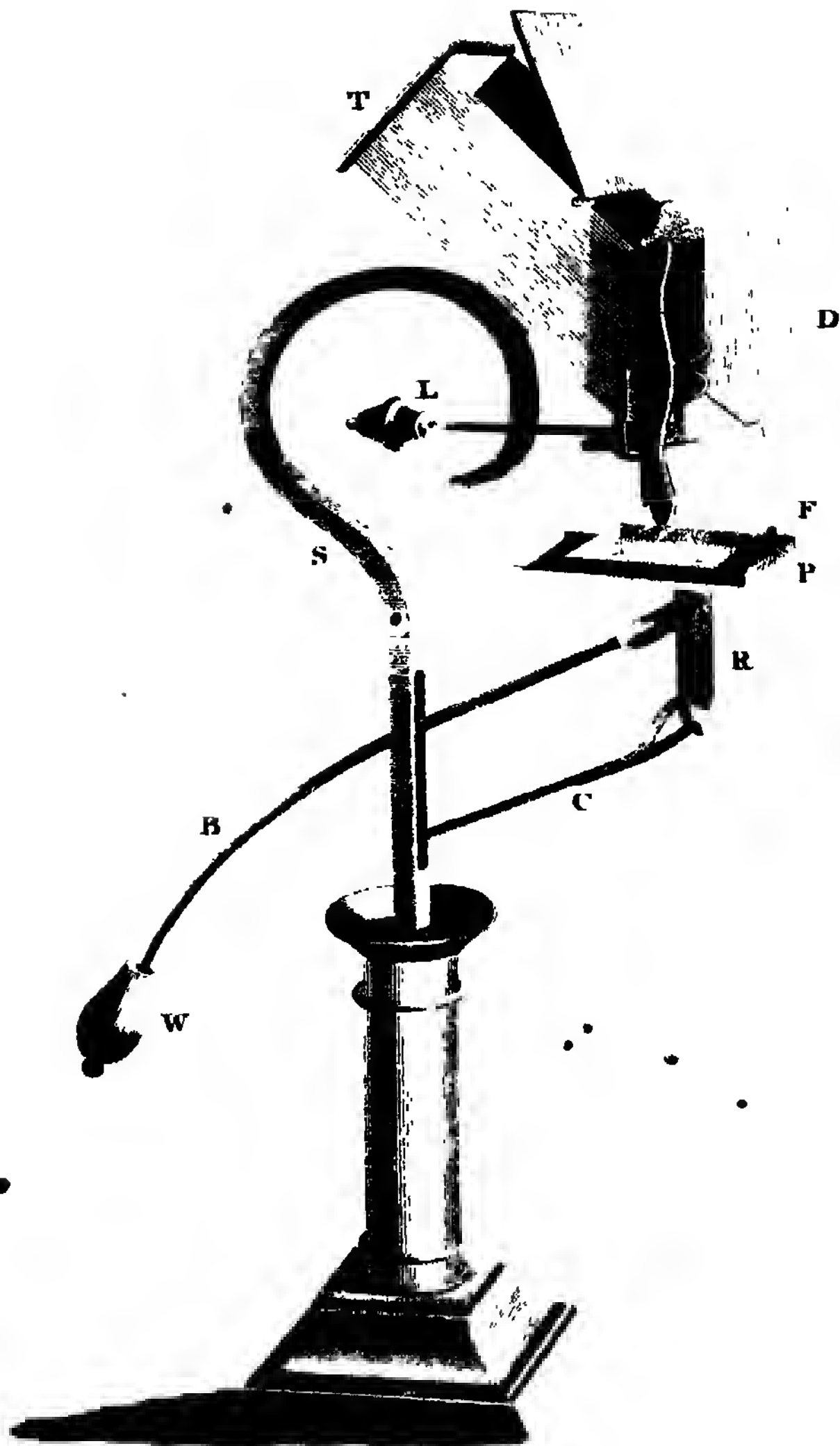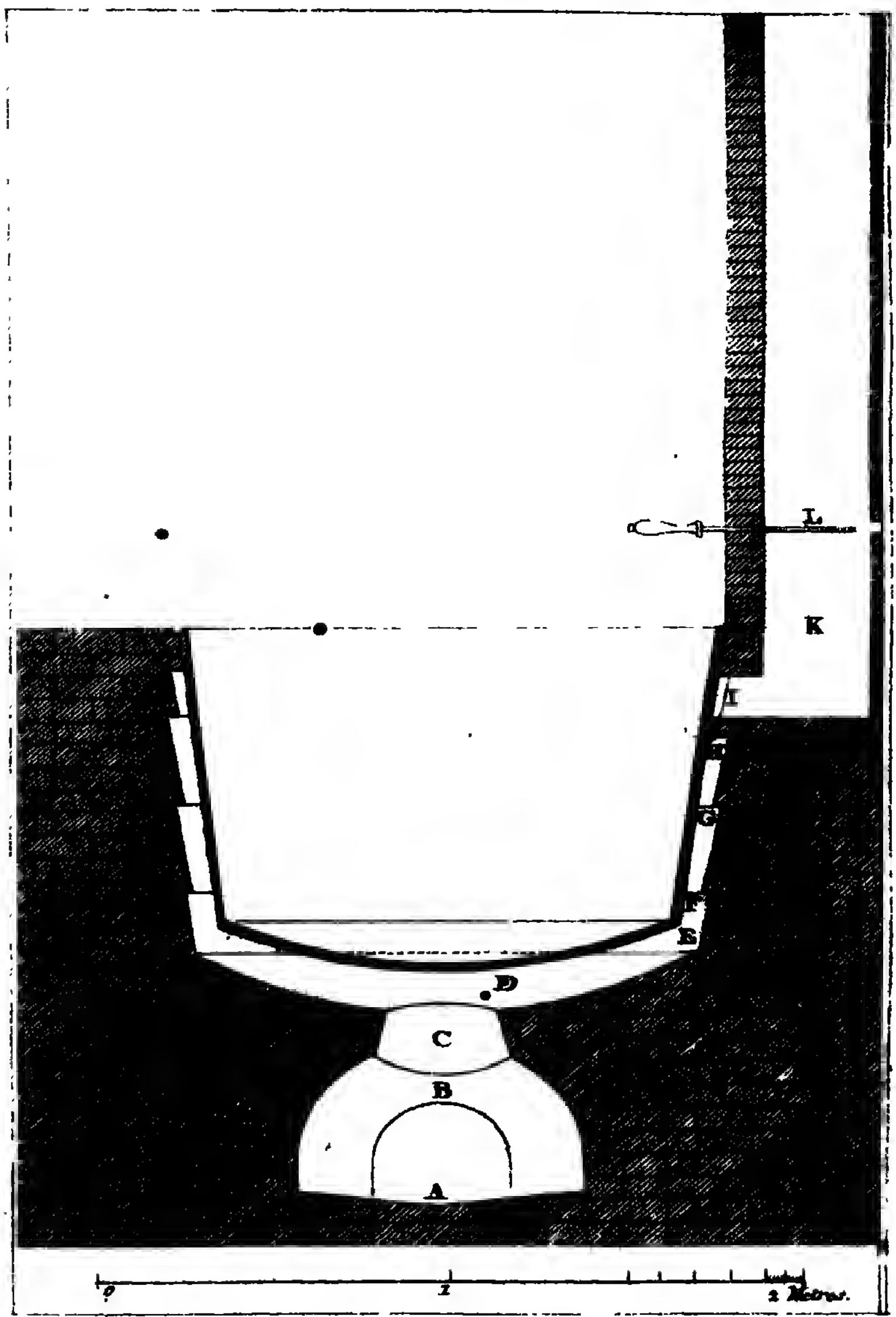
Perpetual Motion.
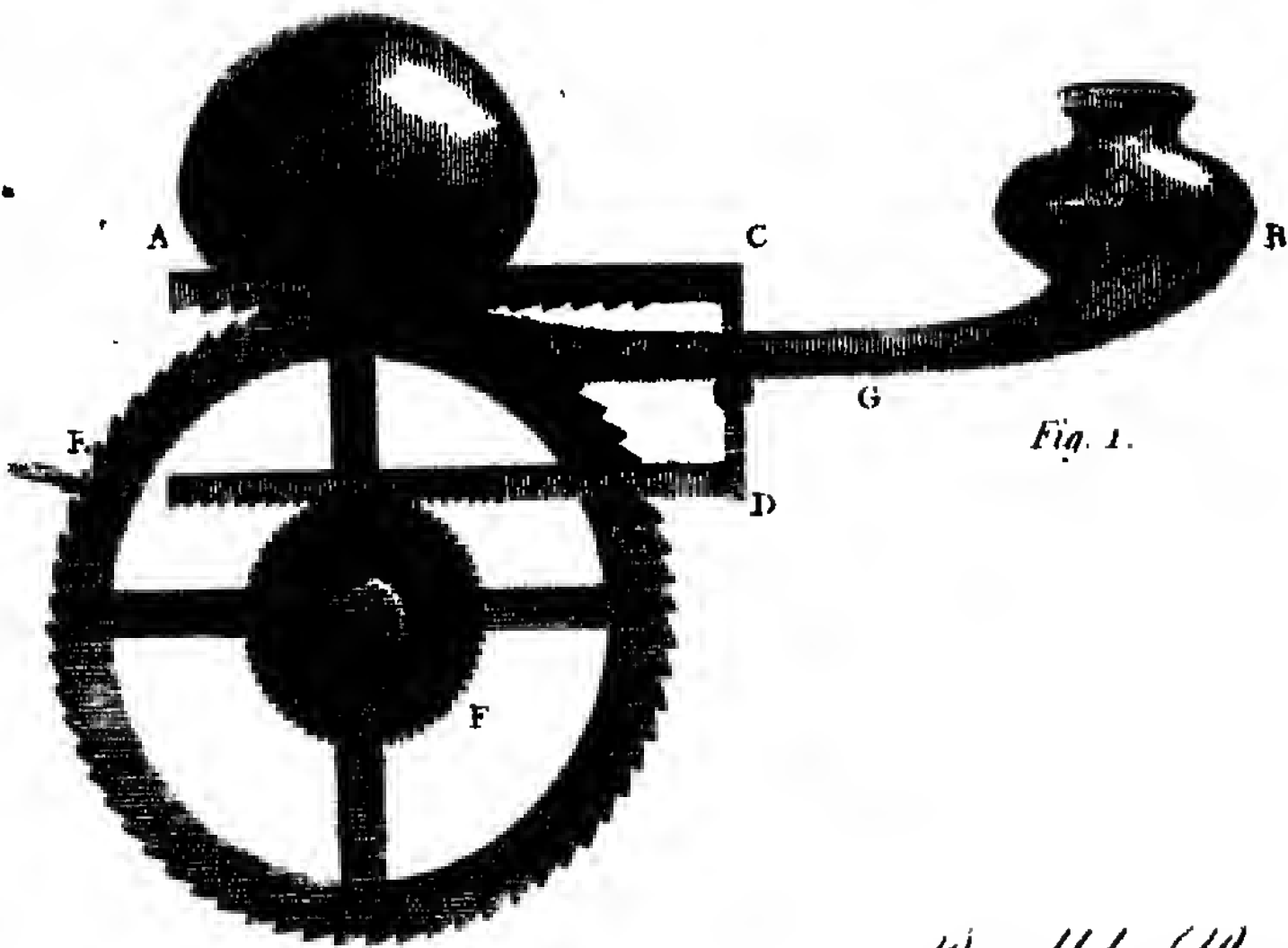
Fig. 1.

Parallel Ruler.
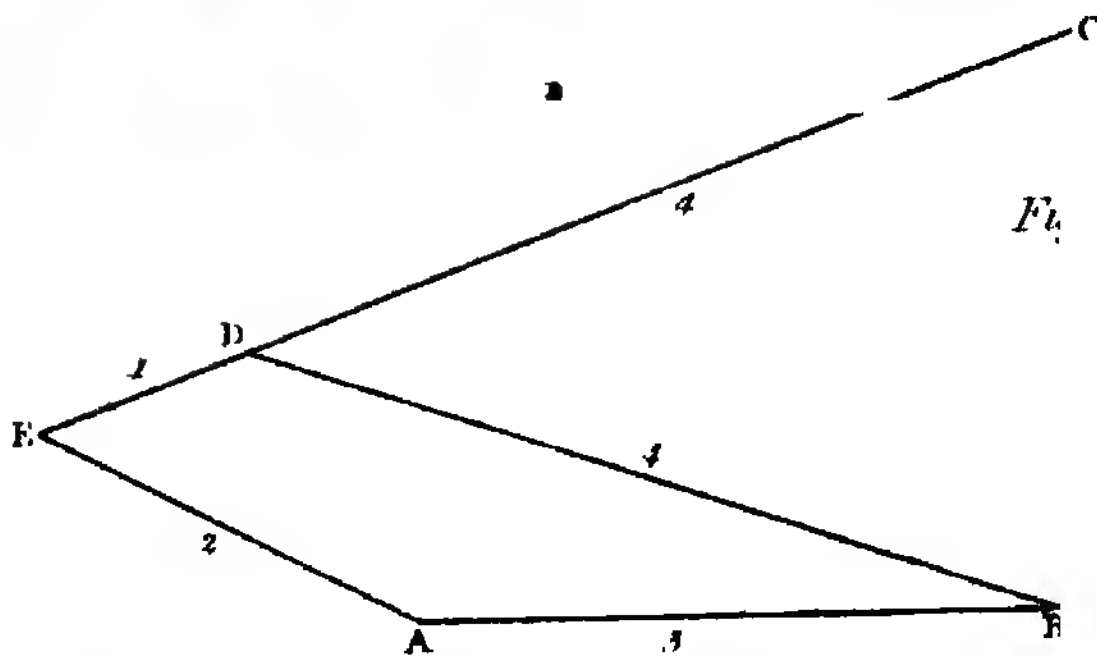
Fig. 3.
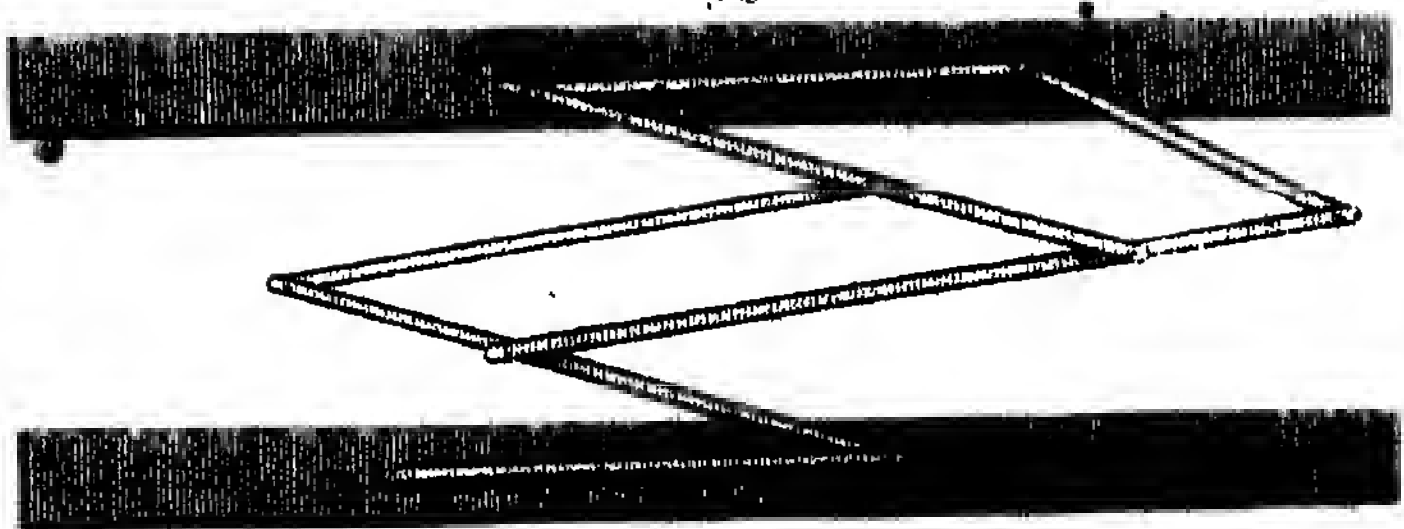
JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

ARTICLE I.

Defcription of a Tubular Pendulum; having all the Properties of the Gridiron; but being more compact as well as more fteady in its Motions. In a Letter from Mr. EDWARD TROUGHTON, *the Inventor.*

To Mr. NICHOLSON.

DEAR SIR,

EVER fince the commencement of your Journal, I have often thought of regiftering therein, the improvements which I frequently make in aftronomical and other inftruments. No man perhaps had ever a greater antipathy to writing than myfelf, and drawn as I am into the vortex of bufinefs, and working with my own hands from nine to twelve hours in a day, leaves me but little time for literary purfuits; yet in one who values himfelf on account of the originality of his ideas, it muft feem flothful not to commit them to the prefs, which until that is done, are like flowers or fruit growing fencelefs by the way fide, which every one that paffes by may gather. *Advantages of publifhing improvements in philofophical apparatus.*

I always confidered the pendulum of a clock more fuitable to our ftile of workmanfhip and habits of thinking, than to thofe of any other defcription of artifts; and agreeable to this idea, I have made pendulums near twenty years, and I own *Pendulums for clocks are particularly fuited to mathematical workmen.*

Reckoning from the top the firft action is downwards, and confifts of the fpring, a fhort wire 0,2 diameter, and a long wire 0,1 diameter; thefe all of fteel firmly connected, reach down within an inch of the centre of the bob, and occupy the middle line of the whole apparatus. To the lower end of the middle branch, is faftened the lower end of the interior brafs tube, 0,6 in diameter, which terminates a little fhort of the top of the exterior tube, and produces the firft dilatation upwards. From the top of the interior tube depend two wires 0,1 diameter, whofe fituation is in a line at right-angles to the fwing of the pendulum, and reach fomewhat lower than the attached tube itfelf, which they pafs through without touching, and effect the fecond expanfion downwards. The fecond action upwards is gained by the exterior tube, whofe internal diameter juft allows the interior tube to pafs freely through it: its bottom is connected with the lower ends of the laft delcribed wires. To complete the correction, a fecond pair of wires of the fame diameter as the former, and occupying a pofition at right angles to them, act downwards, reaching a little below the exterior tube, having alfo paffed through the interior one without touching either. The lower ends of thefe wires are faftened to a fhort cylindrical piece of brafs, of the fame diameter as the exterior tube, to which the bob is fufpended by its centre.

Fig. 3. is a full fize fection of the rod, in which the three concentric circles are defigned to reprefent the two tubes, and the rectangular pofition of the two pair of wires round the middle one, are fhewn by the five fmall circles. By copying this arrangement, from the elegant conftruction of your own half feconds pendulum (Phill. Journal for Auguft, 1799) I avoided much trouble, which muft have occurred to me, unlefs indeed, I had been impelled on the fame idea, by the difficulty of contriving the five wires to act all in a row, with fufficient freedom and in fo fmall a fpace. *Fig.* 4. explains the part which clofes the upper end of the interior tube: the two fmall circles are the two wires which depend from it, and the three large circles fhew the holes in it, through which the middle and other pair of wires pafs.

Fig. 5. is defigned to explain the part which ftops up the bottom of the interior tube, the fmall circle in the centre is where the middle wire is faftened to it, the others the holes for the other four wires to pafs through. *Fig.* 6. is the part which

clofes

clofes the upper end of the external tube, the large circle in
the centre is the place where the brafs covering for the upper
part of the middle wire is inferted, and the two fmall circles
denote the faftening for the wires of the laft expanfion. *Fig.* 7
reprefents the bottom of the exterior tube, in which the fmall
circles fhew the faftening places for the wires of the fecond
expanfion, and the larger ones the holes for the other pair of
wires to pafs through.   *Fig.* 8. is a cylindrical piece of brafs,
which fhews how the lower ends of the wires of the laft ex-
panfion are faftened to it, and the hole in the middle is that
whereby it is pinned to the centre of the bob.   The faftening
of the upper ends of the two pair of wires is done by fcrew-
ing them into the pieces which ftop up the ends of the tubes,
but at the lower ends, they are all fixed as reprefented in *Fig.* 8.
I have only to add to this defcription, that the pieces repre-
fented by *Fig.* 7 and 8, have each a jointed motion, by means
of which the fellow wires of each pair would be equally
ftretched, although they were not exactly of the fame length.

In the apparatus thus connected, the middle wire will be
ftretched by the weight of the whole; the interior tube will
fupport at its top the whole except the middle wire: the
fecond pair of wires will be ftretched by all except the mid-
dle wire and interior tube; the exterior tube fupports at its top
the weight of the fecond pair of wires and the bob, and the
fecond pair of wires are ftretched by the weight of the bob
only.

The firft pendulum which I made of the tubular kind, had
only three fteel wires, and one tube above the bob; that is two
expanfions down and one up; and the quantity which one
of brafs falls fhort to correct two of fteel, was compenfated for,
by extending thofe branches of the rod below the bob, and bring-
ing up an external tube to which the bob was affixed.   There
is an aukwardnefs in this conftruction, owing to the rod reach-
ing about 13 inches below the lower edge of the bob, other-
wife, it is not inferior to the one firft defcribed.

Difficulty of
preventing flex-
ure and ftarting
motion in the
gridiron,

The principles of the gridiron pendulum, I believe, have
never been queftioned, indeed they cannot be; the difficulty
of conftructing it ftrong enough to prevent lateral flexure
in the lifting bars, and confequent friction in the holes of the
different croffing pieces neceffary to bind it together, which
occafion it to act by ftarts, has been the only obftacle to pre-
vent its general application to the beft clocks.

To

To remedy this evil I contrived the one here treated of, and in my own judgment, have fucceeded as near perfection as any effort of mine ever did. The tubes are drawn ftraight and regular in an engine, in the manner that wire is done: and their ftrength affure fuccefs, preventing the poffibility of any fenfible bending, while the freedom of action prevents any irregular motion whatever.

I have no farther quarreled with my own conftruction of the mercurial pendulum, the principles of which are faultlefs, and form elegant; than that it is quite unfit for carriage, and an article too hazardous for me to deal in; but after all, candour obliges me to give the preference to the original one of Graham, which on account of fome fenfelefs criticifms about one part taking heat fooner than another, evaporation of the mercury, &c. was too haftily laid afide. Graham undoubtedly made his pendulum before the expanfions of either brafs or mercury were accurately known; but an ingenious friend of mine, has from the beft experiments, computed the different parts to mathematical precifion, and introduced it a fecond time to the world; and already, there are not wanting proofs of its practical excellence.

Smeaton's pendulum with tubular bob is a good one, but on account of the glafs rod, like my mercurial one, is totally unfit for tranfportation, and like Graham's, is unfightly; while all three labour under the unavoidable difadvantage of not being applicable to the pyrometer in their compound and pendant flate.

I am about to conftruct a pyrometer fit to try the tubular pendulum in its finifhed flate; a pin fixed in the centre of ofcillation, fhall be contrived to act on a nice fpirit level, and fhew the expanfion, if any, in angular meafure: by which I fhall be enabled to adjuft it; for I have no idea of adjufting one by the going of a clock, that complicated apparatus will always, I fear, have too many errors of its own, to be ufed as a criterion of the merits of this more perfect inftrument.

I will here fubjoin the lengths of the different parts of tu- bular pendulums in inches; alfo, the expanfions of brafs and fteel from which I computed them. The length of the interior tube is 31,9, and that of the exterior one 32,8, to which muft be added 0,4 the quantity which in this pendulum, the centre of ofcillation is higher than the centre of the bob; thofe

all

all of brafs. The parts which are of fteel are the middle wire, including 0,6 the length of the fufpenfion fpring, is 39,3, firft pair of wires 32,5, and the fecond pair 33,2. From experiments which I tried in the year 1794, I found my brafs to expand with 60° of heat 0,000640 on an inch, and my fteel 0,0003966. Smeaton obtained under the fame circumftances 0,0006444 for brafs, and 0,0003833 for fteel. Roy found brafs to expand 0,0006316 and fteel 0,0003816.

The above lengths computed from my expanfions will I think be found very correct; if they are tried by Smeaton's, the pendulum will feem to err in excefs, or gain with heat, about 0″,9 a day with 60° of Fahrenheit; if Roy's be ufed, it will err alfo in excefs, under the fame circumftances fomething more than one fecond. The above ftatement fhews with what care experiments of this kind ought to be conducted. All the dimenfions are given in inches and decimals.

If my pyrometrical experiments turn out fuccefsful, they may likely be the fubject of another communication; and I will referve till then my reafons for giving the preference to my own expanfions in the conftruction of the inftrument here defcribed; for this letter, being twice as long as I firft intended, fhall now be concluded by,

<div style="text-align:center">Dear Sir,</div>

<div style="text-align:right">Your obliged and obedient Servant,
EDWARD TROUGHTON,</div>

November 3, 1804.

<div style="text-align:center">II.</div>

Letter from W. Jessop, *Ifq. on an Improvement in the Procefs of blafting Rocks with Gun-powder.*

<div style="text-align:center">To Mr. NICHOLSON.</div>

SIR,

IF I am communicating to you what you already know, you will accept as an apology for it that it is not generally known, and will be interefting to all who are concerned in mining or blafting rocks with gunpowder.

The ufual procefs, after drilling a hole and charging it with powder, is to introduce a wire or fmall iron rod, to preferve a com-

communication with the fuze, and then to ram up the remainder of the hole with stone pulverized by the operation of ramming it; after which the wire is withdrawn and the priming introduced.

This is a tedious operation, often attended with danger, and *is tedious, dangerous and uncertain.* frequently the labour and powder is lost by the priming hole being obstructed in striking out the wire with a hammer.

I had been informed that instead of this tedious operation, *Loose sand proposed instead of ramming.* the blasting had been effected by introducing a straw filled with fine powder, and then filling the hole with loose sand.

I hardly could believe this, conceiving that the part where there would be the least resistence would first yield to the explosion, and therefore that the loose sand must of course be blown out.

But in the month of August last I tried the experiment in *Success of the experiment.* some very hard rock at Fortwilliam and it completely succeeded: I since then have tried it on the lime rocks at Bristol with the same effect.

A few days since, wishing to know how small a quantity of *Oak 20 inches diameter split in pieces under a charge of 3 inches sand.* sand would produce the effect, I caused a hole of $1\frac{1}{2}$ inch in diameter and 12 inches deep, to be bored in a knotty piece of oak of about 20 inches in diameter and charged it with three inches of powder, and upon that four inches of sand, intending (as I supposed that must be blown out) to repeat the quantity of sand inch by inch to the quantity that would be sufficient, but to my great surprize, it split the piece with great violence into six pieces.

I then repeated it on a similar log of oak, with a charge of *The same effect with less powder.* two inches of powder, and *three* inches of sand; it split the piece in two, and sent half of it into the air to the distance of forty yards.

I think it may puzzle a philosopher to account for this: I *Conjecture as to the cause.* have supposed that the sand in contact with the powder, first moving, the particles wedge each other fast before the motion has time to be communicated to the whole.

I have reason for thinking that a much lesser quantity of *Less powder will probably be required in this way.* powder will produce the effect in this way than by the common method; for as it lies loose in its chamber and makes itself more room by a little yielding of the sand, the whole will take fire, and especially if when half the charge is put in, the straw be introduced and then the other half charge, so that it may take fire in the middle of the charge.

That

That the fand will yield a little, I think I may infer from having charged a tube of tin 14 inches in length and 1¼ diameter, with three inches of powder; on exploding it immerfed in water,. I found *five inches* of the tube abruptly cut off, while the remainder was uninjured, but I do not confider this inference as decifive.

For want of time I was prevented from trying how much *lefs* than three inches of fand would have been fufficient.

<div align="center">

I am, Sir,

Your moft obedient,

W. JESSOP,

</div>

London Coffee Houfe,
    Nov. 8, 1804.

P. S. Might not this be ufeful on the evacuation of a fort, in deftroying cannon, inftead of the ineffectual mode by fpiking?

<div align="center">

III.

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On the Mucilaginous Matter of certain Vegetables, and their Ufe as a Subftitute for Gum Arabic: by Mr. Thomas Willis; *being a Continuation of Experiments made upon the Subject by him, in Addition to thofe formerly publifhed in the Tranfactions of the Society of Arts.**

<div align="center">

Letter to CHARLES TAYLOR, *Efq.*

</div>

SIR,

I HAVE taken the liberty of prefenting to the Society for the Encouragement of Arts, &c. fpecimens of the powder of vernal fquills, of white lily-roots, and of falop, for their confideration and trial. I believe the bulbs of the vernal fquill' will be found to be equally ufeful as thofe of the hyacfnthus non-fcriptus; and the ftrong mucilage from the white lily-root and the falop much more fo.

As gum-arabic has been at times, during the laft war, above 30*l.* per cwt. the multiplication of different fubftances that will anfwer the fame purpofe, will affuredly be an object of a very interefting nature to manufacturers in time of war.

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* Extracted from their Tranfactions, Vol. XXI. The former experiments are in our Journal, VII, 30.

<div align="right">

I have

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I have fent all the powder of white lily-root I had left; but am at the prefent time drying fome taken from the garden two or three days ago. As foon as it is powdered, I will fend you a large quantity of it.

I am, Sir,

Your moft obedient Servant,

THOMAS WILLIS.

March 24, 1803.

———————

SIR,

FROM the candour and approbation my paper on the ufe of the hyacinthus non-fcriptus, as a fubftitute for gum-arabic, in fome trials in calico-printing, met with from the Society for the Encouragement of Arts, &c. I refolved to purfue the fubject further; and now offer the following obfervations for your confideration, in hope that trials may be made, that will be found to be of general benefit.

Having frequently obferved, in old gardens, that the vernal fquill grew very prolific, I conceived the root of them might be equally as mucilaginous as the roots of the blue bells. I procured three pounds fix ounces of them on the 8th of July, 1802, and fliced and dried them. They produced one pound one ounce of powder, one drachm of which was diffolved in four ounces of water, by letting the mixture boil a minute or two. When the liquor was cold, there was a mucilage full as ftrong as one made with a drachm of the powder of blue bells, in the fame proportion of water; from which circumftance, I fhould think the vernal fquill will anfwer the fame purpofes as the hyacinthus non-fcriptus. If it fhould, it is a root that may be very eafily and plentifully propagated, and whenever gum-arabic is dear, may be found ufeful. It is to be remarked, that I found no pungency in the powder of the vernal fquill. I have frequently eaten of it, and the tafte is rather agreeable.

Root of the vernal fquill very mucilaginous.

On the 18th of Auguft, 1802, I collected four pounds of the white lily root, which yielded, when dried, rather more than one pound of powder. A drachm of it was diffolved in four ounces of water, by gently boiling it a minute or two: the mucilage was much ftronger than that made with the vernal fquill, but fomewhat darker coloured. This root may

White lily root alfo very mucilaginous.

poffibly

possibly answer the same purposes better than those above mentioned. The powder of the lily-root leaves a bitterness on the tongue.

Gum obtained by solution and evaporation.

Twelve ounces of the fresh roots of the white lily being bruised and pressed, yielded by evaporation one ounce and one quarter of brown gum.

There being a small quantity of it, I made no trial with it; but very probably the expressed juice might be used by the calico-printers with advantage. These roots can be procured at all times, and propagated to any extent; but it must be observed, that these bulbous roots are stronger when they are without stems, or only beginning to shoot out leaves; and the present time is as proper as any that can be for procuring them.

Comfrey root.

I tried comfrey-root; but the dark colour of the cuticle of the root affected the solution, by making it of a dark dirty brown.

I do not pretend to claim any merit in making the above experiments. Every one who is acquainted with these roots, well knows that they are all mucilaginous. My design is only your patronage, to render them useful in the arts, that their virtues and effects might not lay dormant, but be rendered serviceable to trade.

Powder of salop root affords very strong mucilage; probably cheaper than gum-arabic.

After making the above trials, the powder of salop-root was used, by dissolving one drachm of it in four ounces of water, in the manner above mentioned. It produced a very strong mucilage, and, when cold, was a perfect jelly, and much clearer than either of the other solutions. I am greatly of opinion, that the powder of this root will not only answer all the purposes of gum-arabic, but will be found full as cheap, if not cheaper, in proportion to its strength, than gum-arabic; but this is submitted to your consideration and trial.

I have sent some specimens of the Powder of the Vernal Squill, of White Lily Root, and also of the Salop Powder, in order that they may be tried by your direction; and if they should be found of benefit to commerce, nothing would give me greater pleasure, than to find my slender abilities have been of utility to my country, and honoured with the approbation of the Society.

I am, Sir,

Your obedient servant,

THOMAS WILLIS.

March 22, 1803.

Examination

IV.

Examination of Mr. Ezekiel Walker's Experiments and Theory of the Enlargement of the Horizontal Moon. In a Letter from C. L.

To Mr. NICHOLSON.

SIR,

YOUR correspondent Mr. Ezekiel Walker, has favoured the world with a new attempt to account for the apparent enlargement of the horizontal moon *, in which, after indulging his wit, and expressing his astonishment at the expence of those who have laboured on the same subject, he proceeds to assure us that the *pictures on the retina are not permanent but vary as the dimensions of the pupil vary.* ^{Assertion of Mr. Walker that optical images vary with the pupil.}

That you, Mr. Nicholson, who are in some respect accountable to the world for what you admit into your Journal, and who possess a just celebrity for the manner in which you have executed the task,—that you should have omitted to favour the writer and your readers so far as to reject his paper, is to me no otherwise to be explained than by referring it to your impartiality; which may have induced you to leave the refutation of alledged facts to your correspondents rather than enter into a discussion respecting them yourself.

I cannot for a moment suppose you to have had a doubt concerning this imaginary novelty in optics. If Mr. Walker's position were true, the magnifying powers of the same telescope would vary with its aperture; a well illuminated theatre or room would become contracted in its apparent dimensions; the paper at which I now look would suddenly become larger when I cover the candle with my hand; and in a word we should have no certainty if the magnitudes of things were to appear different with every change of the pupil. ^{Consequences that would follow if it were well founded.}

None of these things do in fact happen, and it seems almost trifling to insist upon them. Still less is it necessary to have recourse to lines and figures: but for the sake of your less instructed readers I will meet Mr. Walker by correct experiments of the same description as his own. ^{Experiments of Mr. Walker repeated.}

* Philof. Journal, IX. 164.

Exp.

**The image
formed by a lens
with a broad
aperture is the
fame as**

Exp. 1. I took a clear double convex lens fupported on a
ftand, and placed the fame oppofite an Argand's lamp; fo
that a good image of the flame was formed upon a paper fixed
againft the wall of the room. From the flame to the lens the
diftance was 49½ inches and from the lens to the image it was
88 inches. The horizontal breadth of the image was repeat-
edly meafured = 1.58 inches and to a greater degree of pre-
cifion than one hundredth of an inch.

**with a con-
tracted aperture.**

Exp. 2. The lamp and lens were fuffered to remain unaltered
in the fituations they poffeffed in the former experiment; but
a fcreen was placed clofe to the lens, having a circular hole
of one inch diameter concentric with the lens itfelf. The
image was then fainter but very diftinct, and meafured exactly
the fame as before.

In both thefe experiments the papers on the wall were fe-
veral times changed to prevent any deception from the appear-
ance of former meafures; and the meafyres were taken along
the fame horizontal line by the help of a line on the wall.

Hence it was feen that a diminution of the pupil or aperture,
to admit lefs than one thirty-eighth part of the light, made no
difference in the image.

**The experiments
repeated with an
achromatic lens.**

Exp. 3. I took a good achromatic object glafs of Dollond,
having an aperture of 1.9 inch and placed it at the diftance of
52½ inches from the lamp; when it gave a good image on the
wall at the diftance of 70¾ inches from the lens. The hori-
zontal breadth of the image was 1.2 inches.

Exp. 4. When the experiment was repeated with no other
change than that a fcreen with a central hole of 0.3 inches
was placed before the lens, the image proved 1.2 inches as be-
fore.

The two laft experiments were made with the fame care
and precautions as the former. They fhew that no diminutior
of the image takes place when the pupil is diminifhed to admit
lefs than one fortieth part of the light.

**Probability that
Mr. W. was
deceived, and
how.**

I may not conclude, Sir, without making fome obfervations
on the direct contradiction of my experimental refults to thofe
of Mr. Walker's. May I afcribe to him any thing more than
inaccuracy? Affuredly I do not. Neverthelefs, if he mea-
fured fo uncertain a thing as *the length* of the flame of a candle
without being affured by repeated experiments that its perma-
nency was entitled to fome confidence; if he held his fcreen

<div align="right">nearer</div>

nearer to his lens when the light was rendered fainter by his card; if he miftook the greater vifibility of the fmaller image for a proof that it was in the focus of the lens; in a word, if he did not panfe and examine foimer facts before he adopted a conclufion fo repugnant to many of them, I would fubmit to yourfelf and re iders whether he has acted confiftently with the rules of philofophical inveftigation, or has any reafon to prize his own folution beyond that which is founded on the rules of linear perfpective*.

I am, Sir,
Your humble fervant,
C. L.

V.

The Method of preparing Chinefe Soy. By MICHAEL DE GRUBBENS.†

IN the acts of the Swedifh Academy, for 1764, page 38, we find a defcription of the preparation of Chinefe Soy, by the late Captain Ekeberg; but as this defcription is not only incomplete but even deficient in accuracy, fo as not to produce the true Chinefe compound, I am well convinced that Mr. Ekeberg muft have been unacquainted with the actual procefs. It is moft likely that he depended upon information given him by the Chinefe who are not always to be trufted; a fact of which I have had fufficient reafons to be convinced, during

Ekeberg's account of foy is erroneous.

* Mr. W. having fuppofed that the want of land objects muft take away the notion of diftance at fea, reminds me of an incident concerning the Panorama of Black-Fiiar's Bridge. This picture was exhibited with indications of a confiderable wind with waves on the river, at the fame time that the buildings on the London fide were feen reflected in the water. I took notice of this inconfiftency, and the ingenious author replied that the water had been at firft painted as if fmooth; but it was found neceffary to put in the waves in order to give diftance by their regular diminution in perfpective, and that the reflections were kept in from a notion that they would rather add to the effect than offend by their want of perfect truth.

† Extracted from the Memoirs of the Academy of Sciences of Stockholm for 1803, by M. Linborn, and inferted in the Annales de Chimie, Vol. L. from which the prefent tranflation is made.

my

my five years refidence in their country, in my attempts to af-
certain correctly the manner of treating a fpecies of filk-worm
which affords its thread five or fix times in the year, as well as
their methods of dying cotton and filk and feveral other branches
of domeftic œconomy.

Having fince received for a confiderable remuneration true
notions concerning thefe objects, I faw how far the former re-
citals had differed from the truth. The fame thing happened
in my attempts to difcover the preparation of foy; but as I at
laft became perfectly acquainted with it, I think it proper to
communicate the fame to the academy.

Ingredients of
foy.

Soy is prepared with a fpecies of haricots (which are white
and fmaller than thofe of Turkey) wheat flour, common falt,
and water. The following are the proportions; 50 pounds of
haricots, 50 pounds of falt, 60 pounds of wheat flour, and
250 pounds of water.

Procefs.
Hericots are
boiled in water,

After having well wafhed the haricots, they are boiled in
well-water in an open veffel for fome hours, or until they have
become fo foft as to be worked between the fingers. During
the boiling they muft be kept covered with water to prevent
their burning, and care muft be taken not to boil them too
much, becaufe in that cafe too much of their fubftance would
remain in the water of decoction. The haricots being thus
boiled, are taken out, and put into large fhallow wooden vef-
fels, which in China are made of thin ftaves of bamboo, two

—then mixed
with wheat
flour,

inches and a half in depth, and five feet in diameter. In thefe
they are fpread out to the depth of two inches, and when they
are cold enough to be worked with the hand, the wheat flour
is gradually thrown in and well mixed with the haricots, until
the whole of the before-mentioned quantity has been ufed.
When the mafs becomes too dry, fo that the flour does not
mix well with the haricots, a little of the hot water of decoction
is added.

—and thinly
fpread out in a
veffel covered
up.

The whole being well mixed, the mafs is then fpread abroad
in the veffels before-mentioned. taking care that its depth fhall
not be more than an inch or an inch and a half; and the mafs
is then covered by a lid which fits exactly. When the mafs
begins to become mouldy and heat is difengaged, which hap-

A peculiar
mouldinefs is
fuffered to come
on by admitting
the air:

pens after two or three days, the cover is then raifed by put-
ting two fticks beneath it, in order that the air may have free
accefs. During this time a rancid odour exhales, and if the

mafs

mafs becomes green, it is a fign that the whole goes on properly; but if it begins to he black, which muft be carefully noticed, the lid muft be elevated ftill more, in order that the mafs may have more air. If it once becomes black, the whole is fpoiled.

As foon as all the furface is covered with green mouldinefs, which ufually happens in eight or ten days, the cover is taken off, and the compound is expofed to the fun, and the air for feveral days. When it has become as hard as a ftone, it is cut into fmall fragments, which are thrown into an earthen veffel, upon which the 250lbs. of water, having the 50lbs. of falt firft diffolved in it are poured. The whole is then well ftirred together, and notice is taken of the height at which the water ftands. If it be not convenient to put all the mixture into one veffel, a number may be ufed, taking care that the materials be proportionally diftributed in each. The veffel thus filled is placed in the fun, and its contents ftirred up regularly every morning and evening, and a cover is put on at night to defend it from the cold, as well as to prevent any rain from finding entrance either by day or night. The hotter the fun the fooner will the foy be completed. The procefs is feldom undertaken but in the fummer, notwithftanding which it lafts two or three months.

—the compound is then dried,

cut in pieces and mixed with falt water;

and in a veffel placed in the fun and daily agitated

for two or three months.

As the mafs diminifhes by evaporation, well-water is added; and this digeftion is continued till the falt water has entirely diffolved the flour and the haricots. The veffel is ftill left for fome days in the fun, in order to compleat the folution ftill more effectually, as the good quality of the foy depends upon this circumftance; and the daily ftirring or agitation is continued to the very laft.

When the fluid has become inomogeneous[?] moth and homogeneous

When at length the mafs has become very fucculent and oily, the whole, as well the thick as the more fluid portions, is poured into bags, through which the foy is preffed, and is then clear and ready for ufe. It is not afterwards boiled, as Mr. Ekeberg pretends. It is to be kept in bottles well corked. The Chinefe who deal in this article keep it in large pitchers well clofed. Before it is ftrained in the prefs the foy is of a deep brown colour, but it afterwards becomes black.

it is ftrained by preffure and is foy.

The Chinefe alfo prepare two kinds of foy from the dregs which remain. The firft time they add 150lbs. of water and

Inferior foy from the dregs.

30lbs.

30lbs. of falt, and after having preffed the mafs, they again add 100lbs. of water and 20lbs. of falt, always proceeding as before defcribed.

The two laft kinds of foy are not ftrong, but very falt, more efpecially the latter, which is alfo lighter coloured. Thefe two kinds are the moft common in China, and are ufed both by natives and Europeans. The differences of price are as 8.4.1.

This procefs has been often verified,

In this manner I prepared in 1759, at my refidence in Canton, the whole of the foy I made ufe of, and I alfo brought feveral bottles with me to Sweden. It was fucculent, oily, moderately falt, and very different from what is commonly fold in Europe. With regard to its tafte, it might be put in competition with that of Japan, which is generally confidered as the beft.

by the writer himfelf.

This defcription is more particularly to be depended upon, as I always made the preparation myfelf; and I can venture to affert that this procefs is the only one for obtaining foy of the beft quality.

Soy contains no fpice or fugar, and is fold in China for three half pence a pound.

Mr. Ekeberg affirms that the foy is boiled with the addition of fugar, ginger and other fpices; but this is without foundation, and cannot be true, for a lb. Chinefe of foy cofts no more than two candareens of Chinefe money *. This was the common price during my whole refidence at China, and is too low to admit of thefe ingredients in the preparation. It is alfo the fact that foy has no indication either of fugar or fpice in its tafte; its principal charaĉter is faline.

VI.

On the Laws of Galvanifm.   In Letters from C. WILKINSON *and* THOMAS HARRISON, *Efqrs.*

Windermere Lake, Weftmoreland, Nov. 8.

DEAR SIR,

Some account of Mr. Gough.

I TAKE the opportunity of a few days relaxation in this romantic and picturefque fituation, to trouble you with fome

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* The Chinefe pound or tael is, I think, about 20 ounces, and the candareen is a little more than three farthings, that is to fay, it is one tenth part of the mace which is valued at eight-pence.   N.

galvanic

galvaaic obfervations, which principally originate from a very pleafureable converfation I have lately had with my friend Mr. J. Gough, of Kendal. To you, Sir, this gentleman, as a philofopher and mathematician, is well known. Although deprived of fight in very early infancy, it is amazing to ob- ferve the rapidity with which he proceeds in geometrical in- vefligatious. Independent of this very flriking acquifition, he is the beft botanift and natural hiftorian this country pof- feffes. To thefe branches of knowledge he adds very con- fiderable claffical knowledge; and in his pneumatic enquiries he places no dependance upon any of the gafes which he does not prepare himfelf. To converfe with fuch a perfon is to be improved. To fome lectures I delivered on galvanifm a few days ago, at Kendal, I was honoured with his attendance; and in confequence of the law I was attempting to explain, relative to the powers of galvanifm on metallic bodies, I was favoured, the fubfequent day, with the following letter from a very intelligent and well-informed profeffional gentleman at Kendal, Mr. Harrifon, brother-in-law to Mr. Gough; the particulars of which I dare fay you will find fufficiently in- terefting to merit infertion in your valuable Journal.

To C. WILKINSON.

DEAR SIR, *Kendal, Nov, 5, 1804.*

I MENTIONED to you yefterday my doubts refpecting the Inveftigation of law of galvanifm, which you have laid down in Nicholfon's the power of different piles in Journal for March 1, 1804; where you fay, that the igniting burning wire. powers in batteries of the fame total furface, are as the fquares of the furfaces of the elementary plates, fingly taken in each : •This, if I miftake not, you repeated in your lecture ol laft night; at which time I expreffed my doubts to Mr. Gough, and we afterwards examined the data given in the paper before-men- tioned ; on which we made the following calculations, and found that your conclufion was ftrictly true according to your data, but that the forces of different batteries expofing un- equal furfaces, will be in the ratio compounded of the number of plates in each battery, and the fixth power of the fides of the elementary plates fingly taken in each. The data you have given are the following : 100 plates of four in. fq. ig-

Inveftigation of
the power of
different piles in
burning wire.

nited half an inch of wire; 400 plates ignited two inches; and 50 plates of eight inch. fq. ignited 16 inches of the fame wire.

Now in order to accommodate thefe numbers to algebraic calculation, we will fuppofe a comparifon of three batteries, two of which are compofed of plates of equal diameters, but differing in number; the third fhall alfo differ from the firft and fecond in the magnitude of its plates, and agree with the fecond in the number of them.

Let n be the number of plates in the firft, m the number in the fecond and third batteries; w and y the diameters of the two fets of plates. Then, if the power of any fingle plate bear any conftant ratio to its furface, it will alfo have a conftant ratio to fome power of its diameter, becaufe the furface is as the fquare of the diameter in fimilar figures: let p be that power; let l, k, and z exprefs the lengths of wire which are ignited by the 1ft, 2d, and 3d batteries; then from the firft datum, $n : m :: l : k$, becaufe the effect is as the number of equal plates, according to experiment. Again, feeing that w is the diameter of the plates in the firft and fecond batteries, and y the diameter of thofe compofing the third, and p exprefles the power determining the law of force for plates of different diameters, the force of a plate in the firft and fecond batteries, will be to the force of a plate in the third, as $w^p : y^p$, and as the number of plates in the fecond and third is

<div align="center">Plate. Plates.</div>

equal, we fhall have $m w^p : m y^p :: k : z$, becaufe as $1 : m ::$

<div align="center">Plate. Plates.</div>

$w^p : k$, and as $1 : m :: y^p : z$, then as $w^p : y^p :: k : z$, but as $n : m :: l : k$; therefore $n w^p : m y^p :: l k : k z :: l : z = \dfrac{l m y^p}{n w^p}.$

To apply this general theorem in order to determine the value of p in all cafes, we will begin with your own experiment made with equal furfaces on different numbers.

Since 100 plates, of four inches, ignite half an inch of wire, and 50 plates, of eight inches fq. ignite 16 inches, we have $n = 100$, $m = 50$, $w = 4$, $y = 8$, $l = \frac{1}{2}$, $z = 16$; therefore $100 \times 4^p : 50 \times 8^p :: \frac{1}{2} : 16$; but fince $8 = 2 \times 4$, $8^p = 2^p \times 4^p$, therefore $2 : 2^p :: \frac{1}{2} : 16$, and $16 = \dfrac{2^p}{2^z} = 2^{p-z}$;

<div align="right">but</div>

but $16 = 2^4$; hence $2^4 = 2^p {}^{-2}$; therefore $p = 6$; hence the force of plates are as the 6th power of their diameters.

When the furfaces are equal, the igniting force is as the 4th power of the fides of the individual plates; for in this cafe we have $n w^2 = m y^2$, and $n : m :: y^2 : w^2$; therefore the theorem $n w^6 : m y^6 :: l : z$, becomes $y^2 w^6 : w^2 y^6 :: l : z$; that is, $w^4 : y^4 :: l : z$, which is the law in your particular cafe.

This theorem will be found ufeful in eftimating the force of batteries in general; as an example of which we fhall calculate, by means of it, the force of your intended battery, confifting of 50 plates, each 24 inches diameter. In order to determine the quantity of wire that will be ignited by fuch a battery, we have $n = 100$, $m = 50$, $w = 4$, $y = 24$, or fix times w, $l = \frac{1}{2}$; therefore $100 \times 4^6 : 50 \times 6^6 w^6 :: \frac{1}{2} : z$;

that is, $2 : 6^6 :: \frac{1}{2} : z$, and $z = \dfrac{6^6}{4} = 36^2 \times 9$ inches, or 972 feet.

I am, Dear Sir,

Your's refpectfully,

THOMAS HARRISON.

Stramongate, Kendal.

YOU will obferve, Mr. Editor, from the above theorem, that my ftatement of the powers of two different galvanic arrangements, equal in their metallic furfaces, being as the fquares of the furface of two individual plates, or, which is the fame, as the 4th power of the diameter of each plate, is correct only where the metallic furfaces are equal; when they are not equal, then the powers are as the 6th power of the diameters, or as the cubes of their refpective furfaces.

In a fhort time you will have the pleafure of a paper from Mr. Gough, relative to the decompofition of water by galvanifm: With this view we tried together the following experiments, the refults of which tend to favour the explanation I have attempted in the Elements I fometime fince publifhed.

For this purpofe I employed 270 plates, containing a furface of 6720 fquare inches, and connected with the pofitive and negative fides by wires of platinum. Thefe platinum wires were inferted in a difh of water at eight inches from each other; the pofitive wire difengaged oxigen gas, the negative

R 2   wire

wire hidrogen gas. The decompofition took place very flowly; in proportion as the wires were approximated, the decompofition increafed in the fame ratio.

The platinum wires were then inferted into a capillary tube whofe bore is about $\frac{1}{216}$ of an inch, filled with water; no fenfible effect was produced till the wires were placed at the diftance of half an inch, and then only a folitary bubble became difengaged from the negative wire, never completely detached, but hovering round the extremity, not being fet at liberty by the evolution of another particle.

When the platinum wires were inferted in a tube whofe bore is $\frac{1}{8}$ of an inch, at the diftance of fix inches a flight decompofition was obferved, 'and which neceffarily increafed in proportion as the wires approached each other.

When the platinum wires were inferted in different glaffes of water, and the extremities of a frog deprived of its integument conftituted the connecting medium between the two glaffes, or a wet piece of cloth, at the diftance of three inches a flight decompofition was obferved.

When two wires are connected with the different ends of a galvanic battery, the wires immediately participate, as to their electrical intenfity, with the refpective plates with which they are in contact.

Inveftigation of the laws of decompofition of water by galvanifm.

Let us fuppofe P the wire from the pofitive end of the battery, and N the negative. *Pl. XVI. Fig. 2.* Let us fuppofe that the galvanic arrangement is fuch, that, when the wires are placed at the diftance a, a, the decompofition commences. If the wires be placed at b and b, the decompofition is confiderably increafed; and this increafe will go on, till the maximum will be at d, d, when the wires are infinitely near each other without being in contact.

The pofitive wire, according to its electrical intenfity, produces a correfponding ftate in all fluids with which it is in contact, proportionate to their capacities; fo alfo the negative wire; and it is evident from the principle of electrical charges, that the influence of each muft extend to equal diftances; it will therefore follow, that when at a, a, fuppofing thefe points the apices of two equal cones, the areas of their refpective bafes will be the leaft poffible, and confequently, in their concourfe at i, will only influence a fingle particle of water. When the wires are placed at b, b, then $c i$ will exprefs

prefs the radius of the circular bafe (fuppofing $bc = ai$), and confequently a greater proportion of water fubjected to the influence; and therefore evidently when at d, d, or infinitely near, the maximum of decompofing effects muft take place. This we obferve is confonant to experiments.

If a particle of water confifts of a particle of oxigen and of hidrogen in a flate of chemical union, and if an agent be employed fufficiently fubtle to penetrate the conftituent particle of water, a feparation will take place. Thus caloric will deftroy the aggregation of the conftituent particles of many fubftances, without producing any decompofition; in this cafe it only acts by penetrating the interftices formed by the conftituent particles, without pervading the fubftance of the conftituent particle itfelf. Thus the portions of water, although participating of the refpective electrical intenfities of the two wires, as yet undergo no decompofition; as yet it has not completely pervaded the particle of water. When a particle of water is influenced on one fide by the pofitive wire, and on its other fide by the negative, then the particle of water becomes a fmall Leyden phial; and if the energy of the apparatus fhould be adequate to overcome the refiftance between the two fides of the particle of water, a feparation is produced, and the water becomes refolved into oxigen and hidrogen.

This deftruction of the water will take place at i, the intermediate diftance between a, a; becaufe, when thus remotely fituated, only a fingle particle of water becomes thus operated upon; when at b, b, a greater number of particles of water become thus influenced, and confequently the decompofition is more rapid.

If water placed in a capillary tube be employed, it is evident, however near the platinum wires are approximated, the areas of the bafes of the refpective cones will be not at all increafed, and, confequently, the maximum of its power will be confined comparatively to a folitary particle of water. The inftant a particle of water becomes thus feparated into its elementary portions, it might be expected that thefe portions would be evolved at i, as being the point of concourfe between the pofitive and negative flates; but in this form they are not in the gafeous flate; they require caloric, &c.; and that they do not very readily acquire this other principle

necellary

neceffary to conftitute them gafes, is evident from the oxigen, if an oxidable metal be employed at the pofitive end, entering into combination with the metal, inftead of being difengaged in an aeriform ftate.

Water is a conduĉtor of eleĉtricity; its capacity for this principle being in proportion to its conduĉting powers. When converted into vapour, its conduĉting powers are increafed, and fo alfo in the fame proportion its capacity, as is evident from water, when undergoing this change, abftraĉting electricity from furrounding bodies.

When a particle of water is refolved into its elementary portions, a change takes place in their capacities for eleĉtricity, caloric, &c. at the very nafcent inftant. Oxigen we know, from other experiments, has its capacity for eleĉtricity increafed, and muft acquire this principle before it can affume a gafeous form. This acquifition will be effeĉted with the greateft readinefs in the direĉtion towards the pofitive wire; while the hydrogen particle having its capacity diminiﬂed, will be fimilarly determmed towards the negative wire.

For a future Journal I will trouble you with fome experiments relative to this change of capacity. I am anxious firſt to perufe Mr. Gough's intended paper, from whofe philofophical abilities much may be expeĉted.

Having extended this paper to a greater length than I at firſt expeĉted, I fhall defer, till another opportunity, a reply to Mr. Cuthbertfon's obfervations, and flatter myfelf I fhall be enabled to clearly point out the error into which Mr. C. has fallen.

Cure of rheumatifm and palfy by galvanifm.

Although your Journal may not be deemed the proper medium for any medical communications, as I have lately witneffed the very powerful effeĉts of galvanifm in rheumatic and paralytic affeĉtions. Whatever may prove of benefit to mankind will, I am perfuaded, be readily admitted into your valuable Work.

           I am, Dear Sir,

              Your's, with the greateft refpeĉt,

                  C. WILKINSON,

                     Surgeon, 19, *Soho Square,*

                        London.

Remark

VII.

Remark upon an Affertion of LAVOISIER, *which has been re-peated by eminent Chemifts.* By P. PREVOST, *Profeffor at Geneva, and Correfpondent with the National Inftitute of France.* *

"IT is a conftant phenomenon in nature . . . . . that when any body whatever is heated . . . . . its dimenfions become increafed in every direction . . . . . If after having heated a folid body to a certain point, and by that means feparated its particles more and more, it be then fuffered to cool, thefe particles will approach each other, according to the fame gradation by which they were before feparated ; the body will pafs through the fame dimenfions it before poffeffed ; and if it be reduced to the fame temperature as at the beginning of the experiment, it will refume its original volume. But as we are very far from being able to produce an abfolute ftate of cold ; but on the contrary, as there is no degree of refrigeration which we cannot fuppofe to be capable of being further augmented, it muft follow that we have not yet fucceeded in bringing the particles of any body as clofely together as is poffible, and confequently that *the particles of no body in nature can be in a ftate of contact;* a very fingular conclufion, but to which neverthelefs it is impoffible to refufe our affent."—*Elementary Treatife of Chemiftry, by Lavoifier, Vol. I. at the commencement.*

Affertion of La-voifier, that fince bodies are contractible by cold, their particles are not in contact.

The fame affertion is repeated feveral pages further, where an example is taken of a veffel filled with fmall leaden fhot. "The balls touch each other," fays our author, "whereas *the particles of bodies do not touch,* but are always kept at a fmall diftance from each other by the effort of caloric."

The fame affer-tion repeated,

The authority which this celebrated chemift enjoys in the learned world, is probably the only caufe why this conclufion has been adopted. It is at leaft certain that it is not legiti-mate, though it has been fo often repeated ; and more parti-cularly in a late work, no lefs remarkable for the depth of reafoning and extent of knowledge which it difplays. Ber-

* Annales de Chimie, Vol. L. p. 58.

thollet,

by Berthollet.

thollet, in his *Effai de Static Chimique*, Tom. I. p. 24, parag. 2, * has thefe words : " Cohefion is the effect of that affinity which the particles exercife on each other, and keeps them at a diftance, determined by the equilibrium of this force with thofe which are oppofed to it ; for the property poffeffed by the moft compact bodies of undergoing a diminution of volume by reducing their temperature, proves that there is no immediate contact between their parts."

Apology for Berthollet,

After all, it is not furprifing that an author, who has undertaken and fuccefsfully executed a work of fuch immenfity with regard to chemiftry, fhould refer to one of his moft celebrated predeceffors for the folidity of an argument which relates to the principles of natural philofphy ; and this conduct is fo much the more natural, as the argument never having been contefted, though frequently quoted, a confiderable prefumption arifes in its favour, to difpenfe with any careful examination in cafes where it is not intended to be farther applied. Now it does not appear that any farther ufe is made of the propofition throughout the great work to which our attention is now directed. I therefore confider the paffage here extracted, rather as the occafional mention of a fingular paradox, than as a thefis which the author was defirous of eftablifhing.

whofe authority may neverthelefs affift in eftablifhing this error.

Let this be as it may, it will notwithftanding follow, that the mere incidental mention of an affertion by an author fo juftly refpected, will be confidered as a proof of its truth ; and that the numerous difciples of this great mafter will repeat it with confidence. It is therefore defirable that its want of foundation fhould be infifted on, and that it fhould be directly refuted in a Journal of extenfive circulation ; more particularly amongft thofe who are moft likely to be mifled by fuch an error.

The author does not difcufs the propofition itfelf, but only infifts that expanfion and contraction prove nothing.

Before we proceed, it will be proper to make an effential diftinction. The affertion is as follows : *The particles & bodies do not touch each other;* or in other terms, *there is no immediate contact between the elementary parts of bodies.* Now, to fpeak with propriety, I do not at all intend to deny this affertion, neither do I propofe to eftablifh it. All that I intend to do is, to fhew that it is not legitimately concluded

* See Lambert's Tranflation, Vol. I. p. 2.

from

from the principle whence it has been deduced; namely, the diminution of volume which accompanies depreffions of temperature. Now I fay that this diminution does not at all prove that the elements of bodies have no immediate contact, and confequently that this laft affertion is fo far gratuitous. And this is the whole of what I defign to prove.

If it be true that there are numerous conceptions and examples which are evident and eafy to be given, of bodies fubject to dilation and condenfation without their parts ceafing to be in immediate contact, it muft neceffarily be concluded, that dilation and condenfation can afford no proof in this refpect. For why fhould we refufe to admit fome of thefe conceptions, or to apply fome of thefe examples to the cafe of the elements?

We can eafily conceive expanfion without ceffation of contact.

1. Conceive the particles to be elongated and united by their extremities, like the legs of a pair of compaffes; and they may turn with regard to this point of union, as a centre, and produce condenfations and dilations of the whole apparent mafs of the body fucceffively.

1. Inftance: the legs of compaffes may open and fhut.

2. A dry fponge, or fruit, or mucofity, being plunged in water, will dilate without any ceffation of the fenfible contact; and on the contrary, the fame bodies, when wetted, if expofed to become dry, will undergo condenfation.

2. Sponge, &c. may be compreffed or enlarged.

3. The example of the dilatation of ice and fome metals by cryftallization, an example formally remarked, ftudied, and well explained, particularly by the latter of the two learned chemifts I have cited, muft fhew the falfity of the affertion which I refute.

3. Expanfion in cryftals.

Upon reading the work of Berthollet, and taking notes of fuch things as I was defirous of retaining, or concerning which I faw reafon to doubt, I wrote what is here tranfcribed, i. e. the fummary indication of the three conceptions, or examples, proper to elucidate this fubject.

"I had no intention of fubmitting my remark to its natural judges, when five or fix weeks afterwards, being employed in a very different occupation, I found in the papers of a learned * philofopher, which were entrufted to me at his death,

Similar remarks by Lefage.

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* George Louis Lefage, Correfpondent of the Academy of Sciences of Paris, afterwards Correfpondent of the National Inftitute,
Member

death, a card which contained the fame remark, but in a more abridged form, and by fimple indication. As it offers a variety of examples and conceptions of the fame nature as thofe which I have given, though neverthelefs different, I fhall here give them verbatim :

Note of G. L. Lefage.

" Methods of fhewing that this confequence does not follow :

Expanfion and contraction,
1. Of the hands;
" 1. Four fingers of one hand introduced more or lefs between the four intervals of the fingers of the other hand.

2. Combs, &c.
" 2. The fame with a couple of brufhes, or cards, or combs.

3. Cotton, &c.
" 3. Carded cotton, or hair, and foap lather.

4. Snow.
" 4. Snow.

5. Ofcillation.
" 5. Agitation or ofcillation, which renders the fame particles alternately contiguous and feparate more than a thoufand times per minute."

Conclufion.
The perufal of this note determined me to publifh mine ; not only becaufe I found myfelf authorized by this coincidence, but alfo to render a firft homage to the memory of a philofopher no lefs modeft than ingenious, and to begin in fome refpect to execute his will, by publifhing at leaft one of his notes without making any change. The laft conception offered in this laft fhort note, will probably induce fome philofophers to reflect again upon this fubject, and may perhaps lead them towards the opinions of the writer.

Member of the Royal Society of London, and of feveral other learned Societies, died at Geneva, the 28th Brumaire, in the year XII. (Nov. 19, 1803). His principal writings have not yet been publifhed. I fhall foon give a fhort notice, as well as fome details of his literary life, proper to facilitate the perufal of his works, and eftablifh their originality.   P.

VIII.

Account of a Memoir on chamoying of Leather.
By M. SEGUIN. *

M. SEGUIN, who has already publifhed feveral intereft-
ing works on the arts, relating to the preparation of fkins,
has lately read to the Inftitute a firft memoir on chamoying,
from which we fhall give an extract.

The author ftates, that the art of chamoying confifts in dif- Mechanical
poling the fkins to receive the oil; in impregnating them with operations of
chamoying.
it by different operations, of which he probably referves the
details for a fecond memoir; in then caufing them to undergo
a fpecies of fermentation; in expofing them to the air; and,
laftly, in taking from them, by means of potafh, the excefs
of oil which is ufelefs to them.

He afterwards paffes to the chemical examination of a cha-
moyed fkin.

He found that this fkin did not undergo any alteration by Chemical exa
a long boiling in water; but that, if any acid whatever is mination of cha-
moyed leather.
added (M. Seguin made ufe of fulphuric acid), the fkin dif-
appeared entirely; that a certain quantity of a concrete oil
fwam on the furface of the liquid; that the liquor contained
gelatine; and that, by its evaporation, it depofited cryftals
of fulphate of potafh. He alfo afcertained, that on pouring Gelatine poured
gelatine into a folution of foap, an infoluble precipitate is ob- into a folution of
foap, forms a
tained, which, treated with an acid, comports itfelf exactly precipitate which
like a chamoyed fkin. refembles a cha-
moyed fkin in
Thefe refults, and the confiderations to which the expofi- its chemical pro-
tion of the principal operations of chamoying have given rife, perties.
induced M. Seguin to conclude that the fkins, in the fermen- Theory of the
tation which they undergo, yield a part of their oxigen to the operation.
oil; that the potafh employed to cleanfe them, forms a foap
with the oxigenated oil; that one part of this foap, by com-
bining with the difoxigenated fkin, produces the infoluble
fubftance which makes the fkin chamoyed; and that the other
part ferves to conftitute that greafe which remains in the
leather.

* From Bulletin des Sciences, Tom. III. p. 209.

This

This work is the more interefting, becaufe hitherto no one has confidered chamoying in a chemical view, and becaufe the analyfis to which M. Seguin has fubmitted his refults, renders it fufceptible of acquiring that perfection which the difcoveries of this chemift have already given to the art of the tanner.

IX.

Analyfis and Decompofition of a Liquor employed to render Stuffs impermeable to Water. By M. VAUQUELIN. *

IT is known that, for fome years, different perfons have been fuccefsfully employed in rendering ftuffs impermeable to water, an object of great importance in the clothing of foldiers and failors.

The inventors of this procefs have hitnerto kept the means they make ufe of fecret : there was only reafon to fuppofe that fome fat oil was the bafis of their receipts, but experiment has not yet developed it.

A bottle of this preparation, of which the efficacy was known, having accidentally come into my hands, gave me a defire to inveftigate its compofition ; but before ftating the plan which I followed with this view, I fhall defcribe its phyfical properties.

Phyfical characters of a liquor for rendering ftuffs impermeable to water.

It is a white liquor, milky and opake, of a bitter tafte, and fmelling like foap ; on its furface there is a fpecies of cream refembling that of milk, and it ftrongly reddens the tincture of turnfole. From thefe properties I thought it was fimply a folution of foap, of which it ftill retained the fmell and tafte, which had been decompofed by an acid ; but fubfequent experiments foon convinced me that there was fomething more in it.

Chemical examination.

A turbid part is feparated by repeated filtiation.

Firft Experiment.—To find whether I could feparate the white matter which rendered the liquid turbid, by filtration, I put a certain quantity of it on bibulous paper : it paffed turbid and milky for a long time, but, by returning it feveral times on the fame filter, I fucceeded in obtaining it as clear

* From Bulletin des Sciences, Tom. III. p. 210.

as water; and I afterwards examined the liquor, and the matter which remained on the filter, feparately.

Second Experiment.—If my conjecture was well founded, I fhould only find in this liquor the bafe of a foap united to an acid, of which there was a fuperabundance. My firft care Contains ful-phuric acid. was to fatisfy myfelf of the nature of this acid, and that which its tafte had already indicated to me, was confirmed by mu-riate of barytes producing in it an abundant precipitate, in-foluble in nitric acid: thus I was convinced that the liquor contained fulphuric acid; but, on the other hand, ammonia Ammonia forms having produced in this liquor a white, flocculent, femi-tranfparent precipitate, I faw that it contained fomething more than the falt refulting from the decompofition of foap.

Third Experiment.—I then precipitated a certain quantity of this liquor; I wafhed the precipitate and dried it: as it had all the phyfical charaêters of alumine, I combined it with fulphuric acid; I added to it a little fulphate of potafh, and, by a flow evaporation, obtained fome very fine alum. We Contains alu-mine, probably have therefore already found in this liquor alumine and ful-phuric acid, doubtlefs combined in the ftate of alum. alum;

Fourth Experiment.—It was now requifite to know whether the liquor from which I had feparated the alumine, did not ftill contain fome other fubftance, and I therefore fubmitted it to fome trials by the re-agents, among which the oxigen-ated muriatic acid and the infufion of nut-galls enabled me to difcover a new body: the firft rendered the liquor turbid, and alfo an ani-mal matter, and foon afterwards produced white flakes in it; the fecond produced yellowifh-white flakes, in much greater abundance than thofe arifing from the effeêt of the muriatic acid; whence I fufpeêted that, befides the matters mentioned above, this liquor contained an animal matter, and more particularly ge-latine.

Fifth Experiment.—To be better fatisfied of the nature of this fubftance, I evaporated the liquor to drynefs, by means of a gentle heat; I obtained a yellowifh falt, of a bitter tafte, which, by being re-diffolved in water, left a voluminous yellow matter, in the form of flakes, very glutinous, and ac-quiring, by drying, a fort of elafticity. This fubftance, placed on burning coals, fwells and exhales white fumes, which have the odour of ammonia and fetid oil, fuch as are generally given by animal matters.

I no

Which is gelatine.

I no longer doubted that a certain quantity of animal gelatine had been put into this compofition, with the intention of giving greater vifcofity to the liquor, and enabling it to fupport, for a longer time and more effectively, the parts of the oil in fufpenfion. It is probably by heat and by a commencement of decompofition, that the animal gelatine had become infoluble in water; but I perceived that the liquor containing the falt alfo held fome of it in folution, for the muriatic acid and infufion of galls ftill formed precipitates in it, only lefs abundant than the firft time.

Examination of the fat fubftance;

Sixth Experiment.—In this experiment I endeavoured to learn the nature of the fat fubftance retained in the filter, and of which I have fpoken above: my intention was principally to difcover whether it held fome other fubftance in combination.

For this purpofe I burned it with the filter in a platina crucible; it exhaled a vapour fimilar to that of tallow or the oils; it left fome afhes, of which the filter had furnifhed a part: I found a fmall quantity of alumine in them, which could only proceed from the oil, for the bibulous paper did not contain an atom. I am alfo of opinion, that, in addition to the alumine, this oil contained a fmall quantity of animal matter, but I cannot pofitively affert it.

which retained fome alumine in combination.

Thus, notwithftanding the excefs of acid which exifted in the liquor, the oil in precipitating carried with it, and retained in combination, fome alumine, and probably fome animal gelatine.

It is a combination of oil, alumine, and animal gelatine;

Hence the fubftance which, by unitirg with ftuffs, renders them *impermeable* to water, is not the oil alone, but a combination of this fubftance with the alumine, and probably with the animal gelatine, which renders this property more durable.

and yields a little foda and fulphate of potafh.

Seventh Experiment.—The liquor which I had fucceffively deprived of the oil, the alumine, and, in part, of the animal matter, by the different proceffes mentioned above, gave me, by a flow evaporation, cryftals of a falt compofed of foda and fulphate of potafh.

Another analyfis.

Eighth Experiment.—I made a better analyfis of this liquor by another procefs, which I fhall detail very briefly.

I precipitated the alumine and the oil by lime-water; I collected, wafhed, and calcined the precipitate: that which remained in the crucible was alumine and lime.

        The

The liquor, from which thefe matters had been feparated, evaporated to a certain degree, yielded fulphate of lime, a certain quantity of animal matter, become infoluble by the deficcation of the liquor, and, finally, fulphate of foda and potafh, containing alfo animal gelatine, foluble in water.

The following is the manner in which I conceive this liquor was prepared, *with the exception of the proportions:* Soap and glue, or any other gelatine, was diffolved in water; a folution of alum was mixed with the folution of thefe fubftances, which; by its decompofition, formed in the mixture a flocculent precipitate, compofed of oil, alumine, and animal matter; afterwards fome dilute fulphuric acid was added, to rediffolve the alumine in part, tô render the precipitate lighter, and to prevent it from falling: but the alumine being once combined with the oil and animal matter, will not diffolve again entirely in the fulphuric acid, which is the reafon why the oil continues to be very opake, and neither rifes nor precipitates: it will be underftood, that too much fulphuric acid is not to be added. I do not know if this is precifely the manner in which it is made; I only know, that by following this procefs, I fucceeded in compofing a liquor exactly refembling it, and which poffeffed the fame properties.

(margin note: Manner of preparing the liquor.)

X.

New Experiments on Abforption by Charcoal, made by Means of a new Machine. By C. L. Morozzo.*

1. IN the fecond memoir which I publifhed on charcoal, in 1783,† I mentioned my intention of employing its property of abforbing the gafes, to form a good eudiometer. I immediately fet about the conftruction of an inftrument with which I made fome experiments; but the agitation of the times obliged me to abandon my purfuits: a philofopher, however, to whom I had fhown this inftrument, took the liberty of publifhing it, notwithftanding its imperfections, and without naming the

(margin note: Firft notice of the invention.)

* From the Journal de Phyfique, Floreal, An. XII.
† Journal de Phyfique, p. 376.

author.

author *. Let us pafs over thefe unpleafant events, as well as the alterations and corrections which I made to bring the inftrument to its prefent ftate: the defcription which follows, and the figure will be fufficient to fhow what it now is.

Defcription of the Inftrument. (Plate XIV.)

Defcription of
the inftrument.

II. A B is a tube of clear glafs, three lines and a half in diameter and eighteen inches high: to this tube a ftop-cock C is fixed, to fhut the communication, by means of the key D, fo that the tube may be filled with water or mercury, and the gas to be examined may be afterwards paffed in. To this ftop-cock C a large one E is foldered, an inch and a half in diameter, and almoft three inches long. The handle F F ferves to turn the key of the ftop-cock E; this key has a hole, one inch and three lines in depth, and feven lines in diameter, *(Fig. 2.)* in which the charcoal is placed. This key is perfectly tight, and, when turned, communicates with the cock C, *Fig.* 3, not permitting the paffage of any air whatever. The large ftop-cock is enclofed in a wooden frame, and well fecured by means of the nut H, which confines the lower end of the ftop-cock E behind the board; the lower extremity of the tube dips into a veffel of mercury I I; the board M M keeps the inftrument perpendicular; N N are pads which ferve to raife the cup filled with mercury; O O is a fcale mounted on a flip of wood, which is graduated to inches and lines. The endlefs fcrew P ferves to raife or lower this fcale, to bring it to the level of the mercury in the cup.

III. After having ufed the inftrument for fome time, it is neceffary to unfcrew the keys to clean them, for, by ufe, duft, charcoal, or afhes will get in, which deranges the inftrument, and fuffers air to pafs.

IV. From feveral experiments, I found that the ftop-cock D C, which is attached to the cryftal tube fhould be of fteel, for on examining the refidual air, or filling the tube with mercury to examine any gas, the mercury will at length attack the copper.

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* The author does not name the perfon here alluded to; and in a note he gives a defcription of this firft imperfect inftrument, referring to the figure fo publifhed. This is omitted, becaufe of little utility and not intelligible without that figure. N.

V. I also think it right to propose a small alteration: it is that the extremity of the tube which is plunged into the bowl of mercury, should be a little more bent, that the residual gas may be transferred with greater ease: this slight variation, however, is not at all injurious to the instrument, because the heights are measured by that of the mercury, the level of which is indicated by the scale prepared for that purpose; its greatest inconvenience will be to make a larger and more cylindrical bowl necessary.

VI. This machine was made by J. B. Piana, a very skilful mechanic; he constructed it with such accuracy, that it pre-served the most perfect vacuum for eight days, which is what I am unacquainted with in pneumatic machines.

Manner of using the Instrument.

VII. Suppose I am desirous of examining the absorption effected in atmospheric air by charcoal. The tube A B is filled with the atmosperic air of the place in which the machine is: the stop-cock D is open, the upper one is closed: I place the cup of mercury under the tube, into which I insert a small glass syphon, to cause the air to take the level of the mercury. If I make use of a gas, I pump out the internal gas of the tube with a glass syringe, to bring the level within the tube to that of the exterior mercury in the bowl. Afterwards, by means of the screw P, I move the scale till its zero is also at the level of the mercury.

I make a piece of charcoal, weighing, half a dram or thirty-six grains, red-hot, and with tongs place it in the cavity G of the large stop-cock; I then turn the handle F F, and in a few minutes the mercury will be seen to ascend more or less in the tube, according as the air is more or less pure.

If it is wished to examine other gases, or atmospheric air brought from another place, then close the key D, and fill the tube A B with water; afterwards displace the water with the gas, in the common way by means of a transferrer; bring it to the mercury, and, by means of bibulous paper, absorb the water perfectly; then wipe the outside of the tube carefully with warm flannel, and leave the instrument at rest for two hours: or otherwise make use of a mercurial apparatus, which is preferable.

Inconveniences of the Instrument.

VIII. Nothing is more certain than the property possessed by charcoal of absorbing a more or less considerable quantity of gas, and of atmospheric air; the experiments which I have published are a complete demonstration of it; but I then made use of white glass tubes, hermetically sealed at the upper extremity, and passed the charcoal under the mercury, by which its interstices were filled with that fluid. At present, on endeavouring to make comparative experiments with my new machine, which I was desirous should be a perfect eudiometer, I found that the cavity G, in which I placed the charcoal, and the small space between the two keys of the stop-cock, contained air; that by the heat of the charcoal this air was dilated, and partly expelled, and that consequently, on opening the key of the stop-cock, a small absorption was occasioned by the vacuum thus made, and the mercury then rose. To convince myself, and to discover at what quantity this absorption might be calculated, the apparatus being arranged as for other experiments, instead of charcoal I introduced a piece of red-hot pumice-stone (I gave the preference to this substance, because I was certain that it had not the property of absorbing the air, and also that it would not take a greater heat than charcoal): on turning the key an absorption of an inch took place, and from several experiments, I am convinced that this quantity is uniform within half a line.

Correction of the Instrument.

Correction of the instrument. IX. From the experiment made with the pumice-stone, it is obvious that if an inch is deducted from the total absorption, the real absorption produced by the charcoal will be obtained.

Besides, in comparative experiments this imperfection causes no error; because as they are all conducted in equal circumstances, the variations produced, either by the different gases, or by the different quality of the charcoal made use of, will be always proportional.

X. With this instrument I made several experiments which I shall now relate.

Order of the experiments. XI. I began by examining the action of charcoal when cold, and afterwards at a low heat; I then examined the absorption

by

by charcoal expofed, for a greater or lefs length of time, to the
light of the fun ; this enabled me to afcertain the quantity of
light and caloric which enter into the charcoal.

XII. The experiments on atmofpheric air, which is more or
lefs abforbed according to its purity, follow next.

XIII. After thefe are the experiments on different gafes.

XIV. Oxigen gas gave me very fingular refults, diame-
trically oppofite to thofe I had obtained in tubes hermetically
fealed, and when I paffed the charcoal through the mercury,
which deranged me greatly in the confequences I deduced
from it.

XV. After this article will follow fome confiderations on
the quality of the charcoals made ufe of, which prove that their
property of abforbing is different according to their different
qualities.

XVI. I afterwards fhow that my inftrument may be ufed in
many cafes, if inftead of mercury, coloured water is employed,
but with requifite modifications.

XVII. The charcoals which had been employed in the ex-
periments all acquired a greater weight: I endeavoured to
extract the air which they had abforbed.

XVIII. I alfo mention the trials which may be made by
faturating the charcoal with different fubftances, having only
made four experiments.

XIX. I then proceed to the conclufion, in which I endea-
vour to give the explanation of thefe experiments. The opi-
nion which I had adopted in the fecond memoir, receives ad-
ditional confirmation from it ; but the refults of the experi-
ments I made on oxigen gas having changed the ideas I had
formed, I am not willing to hazard an explanation at prefent.
Philofophers will therefore be contented with having new facts
and new experiments.

, The experiments which I detail were repeated feveral times,
and their accuracy may be depended on, as I can affure my
readers, that I have always confidered truth as the brighteft
ornament of a philofopher.

Experiments made with my new Inftrument.

1ft. The charcoal which I generally employed was that of
beech-wood ; the weight of the pieces half a dram, that is to
fay, thirty-fix grains.

Experiments
with the new
inftrument.

.   S 2   2d. The

2d. The tube of my glafs was eighteen inches long, from the bottom of the key to the furface of the mercury, the level of which is indicated by the moveable fcale. The diameter of the tube is three lines and a half : it contains one ounce and feven drams — five grains of water, or a volume of atmofpheric air of four grains and a half at a temperature of $+10°$.

3d. Being defirous of throwing a light upon the property poffeffed by charcoal of abforbing a part of the atmofpheric air, as well as of feveral gafes, by means of my new inftrument, I made a great number of experiments, which I fhall now defcribe.

4th. Charcoal, when cold, has the property of abforbing fome fmall portions of atmofpheric air, and this abforption, although very flow, is not complete in lefs than twenty-four hours.

On charcoal heated without the contact of fire,

5th. Wifhing to try whether charcoal to which heat had been communicated without direct expofure to fire, would fhow any abforbent power, I placed a piece in a fmall matrafs, which I immerfed in boiling water for an hour; having afterwards put the charcoal into the machine, I obtained an abforption of about three inches.

6th. I boiled oil, in which I left the matrafs for an hour ; an experiment with this charcoal gave an abforption of three inches two lines.

7th A fmall matrafs containing a piece of charcoal was immerfed for an hour in boiling alkaline lixivium : it gave an abforption of three inches three lines nearly.

Thefe three experiments fhow that heat communicated to charcoal, even without the contact of fire, gives it the property of abforbing a larger portion of air.

and expofed to the folar light.

8th. The preceding experiments induced me to examine whether the light of the fun would communicate the fame property to charcoal, I therefore expofed different pieces to this light, in a white porcelain bowl, and the abforptions were proportionate to the times which the pieces were expofed to the light of the fun *.

* At the inftant of putting a piece of charcoal into the machine, I placed another which had been expofed with the reft to the fun for three hours, on the bulb of a thermometer; it only raifed one degree and a half of Reaumur's fcale : the heat therefore was not very confiderable.

After

After two hours the charcoal abforbed one inch two lines.
After three hours, one inch eight lines.
After five hours, two inches.
After feven hours, three inches nearly.

It is difficult to obtain very precife refults, becaufe the heat Th of the fun is greatly varied, according to the wind; but in wit this cafe it is light as light which unites with the charcoal, for in very cold days when the thermometer in the fhade was at — 6°, I ftill obtained an abforption *.

9th. Being defirous of afcertaining whether this abforbent property belonged to charcoal exclufively, I tried feveral other bodies, which there was reafon to believe contained a great deal of the matter of fire.

10th. I took a fmall cylinder of loaf fugar, cold; it did not Experiments on produce the fmalleft apforption.   loaf fugar,

A fimilar piece, which I had heated in a matrafs with boil- ing water, gave an abforption of between two and three lines.

A piece of fugar which had been expofed to the fun for two hours alfo gave an abforption of between two and three lines.

11th. I then proceeded to the examination of fulphur and on fulphur, fealing wax.

A cylinder of fulphur of the fame weight gave no abforp- tion when cold.

A fimilar cylinder, which had been expofed four hours to the light of the fun, gave an abforption of about three lines.

A piece of cold fealing wax did not give the leaft abforption, and on fealing- and a fimilar piece which had been two hours in the fun, gave wax. an abforption of about two lines.

12th. All thefe fmall abforptions which were obferved as well in the fugar as in the fulphur and fealing wax, were only owing to the flight heat of thefe bodies, which produces a di- latation in the air of the cavity, and a difplacing of that por- tion of the air which is filled by the folid body; for a piece of pumice ftone, of cork, or of any other body, produces the

* It would be interefting to examine the abforptions which might be produced by different pieces of charcoal which had been in the light of the fun tranfmitted through coloured glaffes, or by making the different coloured rays fall on the charcoal by means of a prifm, and to obferve whether the piece expofed to the red ray would abforb more or lefs than that which had received the violet ray, and fo of the others.

fame

Charcoal alone possessed of this absorbent property.

same effect. It is therefore in charcoal alone that this absorbent property resides, and it is developed to the highest degree by heat and light.

Experiments on Atmospheric Air.

Experiment atmosph.

13th. I then proceeded to examine the effects of incandescent charcoal on atmospheric air, in my instrument, and I repeated the experiments several times: the air of my garden was constantly less absorbed than that of my bed-room, an hour after the windows had been opened, and this was less absorbed than that of my bed-room in the morning before opening the windows: the absorptions were,

The air of the garden, seven inches six lines.

The air of the room after the window had been open, eight inches one line.

The air of the room, in which it had not been renovated, eight inches six lines.

Former experiments.

14th. In the experiments which I made in the year 1783, with tubes twelve inches in height, and one inch in diameter, I passed the charcoal through the mercury; the absorption of the atmospheric air was then always about three inches and six lines, that is to say, a little more than one fourth: in this case it is uniformly greater and about one third. Probably the mercury filled many of the interstices of the charcoal when it was passed through it, which might prevent this greater absorption.

Air of a privy.

15th. I afterwards examined the air of a privy, which was taken into the syringe seven toises above the sewer.

The absorption was eight inches, that is to say, nearly equal to that of my room. This did not surprize me, because in those compositions which contain azote, this gas is not much absorbed, as we shall presently see: besides, Mr. White with an eudiometer of nitrous gas also found that the air of a privy gave him an absoption equal to that of common air *.

Experiments on the Gases.

Experiments on the gases.

16th. After having examined the atmospheric air, I proceeded to the examination of carbonic acid gas: I filled the

* See Journal de Physique, Tom XVIII. p. 145, and the reflections of Guyton Morveau in his excellent treatise on the means of disinfecting the air.

tube

tube with this gas, which I had extracted from powdered mar- Carbonic acid ble by vitriolic acid. I placed the incandefcent charcoal in the gas. machine which effected an abforption of fixteen inches fix lines, that is to fay, of eleven twelfths of the capacity. This experiment correfponds exactly with that which I made with the tube of twelve inches in height, and one inch in diameter, and in which the charcoal was paffed through the mercury: the abforption in that cafe was eleven inches.

17th. I afterwards examined the effect produced on the fame gas by a piece of charcoal which had remained five hours in the folar light. It caufed an abforption of ten inches and three lines, which furprized me greatly.

18th. But to afcertain whether charcoal has any peculiar affinity with carbonic acid gas, I tried this gas by placing a piece of cold charcoal in the machine, and the abforption was nine inches fix lines.

19th. To difcover, the effect of carbonic acid gas when mixed A mixture of with atmofpheric air, I filled the tube, which is eighteen inches carbonic acid high, with nine inches of carbonic acid gas, and nine inches pheric air. of the air of my room with the windows open : the charcoal effected an abforption of twelve inches and three lines.

On analyfing this experiment, it agrees perfectly with thofe related above, for

The abforption of the air of my room was   8 inches 1 line.
————————— of the carbonic acid gas   16   6

                              Total   24   7

The half of this is twelve inches three lines and a half, within half a line of the abforption obtained.

20th. I afterwards tried the action of charcoal in hidrogen Hidrogen gas. gas, obtained from iron by fulphuric acid, and the abfoption was three inches and one line: it was accomplifhed in five or fix minutes, and ftopped at that height : the effect obferved in my machine did not furprize me, for I had noticed in my firft memoir, that of all the aeriform fluids, hidrogen gas was abforbed the leaft. There is a perfect correfpondence between this experiment, and that which I made in the year 1783, in cryftal tubes of one inch in diameter and twelve in height : the abforption was then two inches and one line, that is to fay, one fixth of the height; in my new machine, the tube of which is eighteen inches long, the abforption was three inches and one line, which is alfo the fixth of the capacity.

21ft. On

21ft. On trying the fame hidrogen gas with a piece of charcoal which had been kept about four hours in the bright folar light, I had only an abforption of fix lines.

22d. I next proceeded to the examination of azote gas. I began by examining that obtained from atmofpheric air, in which a candle was extinguifhed.

The abforption was fix inches.

23d. I examined that obtained by the combuftion of fulphur, and had an abforption of feven inches and three lines : it is to be obferved that this gas always contains fulphureous acid, and for that reafon more of it was abforbed.

A fecond experiment with the charcoal gave me feven inches and two lines.

I afterwards tried the azote gas obtained from atmofpheric air in which I had extinguifhed a large piece of charcoal under a bell : the abforption was feven inches fix lines ; but this gas always contains a little carbonic acid, and confequently there is a larger abforption.

24th. To be fure of having the pureft azote gas without mixture, I took the gas obtained by decompofing nitrous acid by means of hidrogen gas, which laft gas had been obtained from the decompofition of water by iron.

The abforption was only fix inches and one line.

We have not yet very correct ideas with refpect to azote : it appeared to me, however, in feveral of the experiments which I made with atmofpheric air, that when it is united with aqueous vapour, more of it is abforbed than when in a dry ftate. Befides it is evident that the azote gas is not much abforbed, which correfponds with my firft experiments.

• *(The Conclufion in our next.)*

XI.

On the Commerce of Hens Eggs, and on their Prefervation. By
M. PARMENTIER *.

THERE are very confiderable differences in the eggs of hens in refpect to fize. Some are as large as the eggs of ducks, while others refemble thofe of pigeons. M. Parmentier, from

* From Bulletin des Sciences, Tom. III. p. 214.

                                    obfervations

obfervations on a great number of hens of different breeds, which he reared in the fame place, has found that the bulk of the eggs depends much more on the breed of the hen than on the quantity of nourifhment. Thofe breeds which give the largeft eggs are not however to be preferred on that account, becaufe with them as much may be loft in the quantity of eggs as is gained by their fize. Of all the breeds known in France the author gives the preference, with refpect to the produce of eggs, to that which is called the common hen, and which is only common becaufe its merit is known. Thofe which have black legs are in greater efteem than thofe with yellow ones.

From comparative experiments continued for a year, M. Parmentier found that though the eggs of this breed were not fo large as fome others, yet every thing befides being equal they produced at leaft one half more.

Next to this breed of hens comes the crefted hen and the Superior breeds. large Flemifh hen. · The one is more delicate eating, becaufe laying lefs than the common hen, it grows fatter; the other, without being more productive, is preferable for raifing chickens. The *filky* hen *(poule de foie)* fo beautiful in its form and in the finenefs of its plumage, fo careful in laying, fo affiduous in fetting, fo tender of its chicks, might be recommended, but unfortunately two of its eggs are not worth one common egg. This circumftance places it among thofe which muft be left for the curious.

After the choice of the breeds, care muft be taken that the Management of hens are neither fed too abundantly nor too fparingly; that they the hens. do not wet their feet, that they are fufficiently clofe in the rooft to heat and electrify each other; and, that they meet with a little warm dung in the day time.

When the only object in keeping hens is to procure eggs, and thus turn to profit the grain which remains among the chaff and in the dung-hills, it is wholly ufelefs to keep cocks at the fame time, becaufe experience has fhown that hens deprived of the male do not lay lefs than thofe which have it. The faving by this is not the only nor the greateft advantage. The unfecundated eggs keep much better than thofe which are fe- Unfecundated cundated. It has been found by experiment that they can fup- eggs leaft liable port a heat of 32° (87° F.) for thirty or forty days without experi- to fpoil. encing any change. It is therefore evident that the evaporation of the liquors is not, as Reaumur thought, the immediate
caufe

caufe of the putrid change in eggs, and that to preferve them, it is not fufficient to cover them with greafe or oil, as this learned man recommends, fince in the experiment mentioned above the unfecundated eggs did not fpoil, though they loft confiderably by evaporation. The fecundation, by the prin-ciple of life which it communicates to the germ, expofes the eggs to many accidents which do not take place with thofe in which the male has had no fhare.

M. Parmentier particularizes fome of thefe accidents. Some of them arife from the commencement of the developement of the germ. Sometimes it is enough if feveral hens lay their eggs in the fame neft; for the egg which is firft laid in it, par-taking in fuçceffion, and for fome hours, of the heat of the hens which follow each other, undergoes a fpecies of incuba-tion which excites the vitality of the germ, and this egg be-comes changed, though it is but recently laid. It is thus that eggs of the fame age appear frequently to differ in their frefh-nefs. At other times the change in the egg may arife from the fecundating germ being killed, either by thunder, or in the conveyauce, by the jolting of the carriage or the rolling of the veffel, or by the lapfe of time. When the germ is once dead it corrupts, and communicates the corruption to that which furrounds it. This theory appears to explain one me-thod I made ufe of fuccefsfully for preferving eggs even when fecundated: it confifts in plunging them for a couple of fe-conds into boiling water. It is known that by this means they become fufceptible of being preferved for feveral months, if they are afterwards kept in a cool place, or in falt. M. Par-mentier fufpects that the utility of this procefs depends on the deftruction of the vitality of the germ by the boiling water.

Mariners affert that eggs laid at fea keep better than others. May not this arife from the hens on board a fhip having no com-munication with cocks? For the fame reafon the diminifhed vigour of the cocks in our poultry yards, in autumn, may be one caufe of the eggs laid in that feafon being more capable of being kept than thofe of the firft laying, to which may be added, that the hens are at that time fed more with grain and lefs with herbage.

From thefe obfervations, M. Parmentier thinks that the firft condition towards obtaining eggs capable of being preferved

and

and tranfported without fpoiling, is not to keep a cock with the hens in the poultry yard. It is an error to fuppofe that eggs not fecundated have a worfe tafte than thofe which are fo. The author afcertained that the moft delicate palate was incapable of difcovering any difference. Afterwards it is only requifite to fhelter the eggs from humidity, light, heat, and froft. The method which fucceeded beft with the author was to procure bafkets made of ftraw, in which he placed the eggs with layers of chaff between them. The ftraw and the chaff are dry materials, fmooth, very bad conductors of heat, and confequently, very proper to preferve the character of frefhnefs in the eggs: thefe bafkets were afterwards fufpended in a dry, dark and airy fituation.

XII.

Method of giving the Colour, Grain, and Hardnefs of Steel to Copper. By B. G. Sage*.

MARGRAFF and Pelletier have publifhed their inveftigations relative to the union of phofphorus with different metallic fubftances; the French chemift has brought this procefs to perfection: it was by repeating and varying his experiments that I difcovered that the readieft and moft certain method of phof- **The readieft and moft certain method of uniting copper and phofphorus.** phorating copper is to take the copper in the metallic form, to melt it with two parts of animal glafs, and a twelfth part of powdered charcoal. But it is effential that the copper fhould offer a large furface; an advantage which is obtained by ufing chips of this metal, which are to be placed in alternate layers with the animal glafs mixed with the powdered charcoal. I expofe the crucible to a fire brifk enough to melt the animal glafs; it forms phofphorus; the greateft part of which burns, while another part combines with the copper, in which it is fo confolidated that it does not quit it, although it is kept in fufion for twenty minutes under the animal glafs which has not been decompofed.

When the crucible has cooled and is broken, the phofpho- **Phofphorated copper.** rated copper is found under the glafs, which has paffed to the

* From Journal de Phyfique, Meffidor, An. XII.

4 •   flate

state of a red enamel, in the form of a grey and brilliant button : on weighing it, it is found to have acquired one twelfth by this operation.

Its action on polished iron.

If the melted phosphorated copper falls on a plate of polished iron, it spreads itself in the form of plates of various configurations which are iridescent like the neck of a pigeon.

Is more fusible than copper.

Phosphorated copper is much more fusible than red copper : it may be often melted under charcoal powder without losing its properties.

Does not quit the phosphorus without difficulty.

The same phosphorated copper, exposed for a long time under a muffle, does not separate from the phosphorus without great difficulty.

Resembles steel in hardness, grain and colour, and does not tarnish by exposure to the air.

The copper thus combined with the phosphorus acquires the hardness of steel, of which it has the grain and the colour; like it, it is susceptible of the most beautiful polish; it is turned easily in the lathe; it does not change in the air : I have for fifteen years, kept buttons of polished phosphorated copper in my laboratory, which have not experienced the least change. The copper does not develope any smell on being rubbed : if it was ductile it would be of the greatest utility, since fat bodies do not seem to make any impression upon it.

A small quantity of charcoal to be used in its preparation.

In the phosphoration of copper there is only a part of the animal glass decomposed : because a sufficient quantity of charcoal is not employed to convert all the acid into phosphorus; but it is necessary that this should be the case, that the phosphorated copper may separate and collect with facility.

Red enamel.

The deep red enamel which is formed in this experiment may be advantageously employed for porcelain or the enamels, since it does not change colour in the fire.

Copper and phosphorus can only be combined in the dry way.

Copper cannot combine with phosphorus except in the dry way. If a cylinder of phosphorus is put into a solution of nitrate of copper diluted with four or five thousand parts of water, at the end of eight days the copper will be found in a metallic form, crystallized and ductile, forming a case for the cylinder of phosphorus.

Observations

XIII.

Observations on Mr. GOUGH's *two Letters on Mixed Gases.*

To Mr. NICHOLSON.

SIR,

MY reply to Mr. Gough's ftrictures being chargeable, it Introduction. feems, with acrimony and ridicule, and with having but few arguments, and thofe in appearance negligently conducted. (though the laft charge requires a whole letter to make it good); I propofe, in what follows, to avoid the two former of thefe as much as may be, and to be as careful as poffible in conducting my arguments; fo, that if they appear to Mr. G. deftitute of that logical precifion which characterifes his, he may afcribe it to my inability, and not to any want of inclination.

The accuracy of Mr. G.'s demonftration in the former let- Obfervations on ter, depends upon that of three phyfical data : 1ft, The fpe- Mr. Gough's letter on mixed cific gravity of azotic gas; 2d, The fpecific gravity of oxi- gaffes. genous gas; and, 3d, The quantity of oxigen in a given volume of atmofpheric air. If any *one* of thefe be wrong, it may prove fatal to his demonftration : now it unfortunately happens that all *three* are wrong, and that, when corrected, they prove the very reverfe of his propofition; namely, that *atmofpherical air is a mechanical mixture of oxigenous and azotic gafes.*

Mr. G.'s data are,

Sp. gravity of azotic gas,   .985

———————— oxigenous gas, 1.103

Quant. of oxigen in atmo-

fphere, *per cent.* in bulk,   22-25-28, uncertain, which.

The true, or at leaft the moft approximate numbers, are,

  Sp. gravity of azotic gas,   -   .966

   ———————— oxigenous gas,   1.127

  Quant. of oxig. in atmofphere,

   per cent. in bulk,   -   -   21

Then, *per* Mr. G.'s theorem, $21 \times 1,127 = 23.667$

     and $79 \times .966 = 76.314$

         ————

         99.981

* See Pages 107, 160, of the prefent Vol.

           The

The fum is fo near 100, that Mr. G. will not venture to pro-
fume any thing on the difference, further than that the data
are ftill in a fmall degree incorrect, which I believe no body
will difpute with him.

Mr. G. by this time is ready to query, How do you know
your data to be more correct than mine? I will now inform
him.

Dr. Prieftley was perhaps the firft to inveftigate the fpecific
gravities of the two gafes in queftion. His method was very
exceptionable; but fuch is generally the cafe in the infancy
of any fcience. (See. Vol. II. Page 452, abridg. Ed. of his
Nat. Philof.) He found azotic gas as much lighter than
common air as oxigenous was heavier. Mr. Kirwan foon
after gave a much nearer approximation; namely, the one
which Mr. G. has adopted. Lavoifier alfo found the fpecific
gravity of feveral gafes. (See Elements of Chemiftry, Append.
Table 7). His refults nearly agree with Kirwan's in regard
to oxigen, but differ confiderably in regard to azote. Laftly,
Mr. Davy, when inveftigating the compounds of azote and
oxigen, found it expedient to afcertain with precifion the
fpecific gravity of the two gafes. Having every means of
his predeceffors, and their refults before him, he ought at
leaft to have decided in regard to the difference between
them. Accordingly he finds his refults to agree with Lavoi-
fier's in refpect to azote; but he finds the fpecific gravity of
oxigen fomewhat greater than either of them. (See Re-
fearches, pag. 555).

The following table exhibits the refults of all thefe together,
reduced to the ftandard of atmofpheric air.

Specific Gravities of

	Azotic Gas.		Atmof. Air.		Oxigenous Gas.
According to Prieftley,	.989	—	1.000	—	1.011
Kirwan,	.985	—	1.000	—	1.103*
Lavoifier,	.966	—	1.000	—	1.102
Davy,	.966	—	1.000	—	1.127

As for Mr. G.'s third datum, I think it fcarcely pardonable in
the prefent day, for any one to pretend to difcufs a queftion
concerning the conftitution of the atmofphere, under the un-
certainty whether it contains 22 or 28 per cent. of oxigen.
He ought to be acquainted with the hiftory of eudiometry,

and

and to repeat experiments confidered as decifive, efpecially as they are of a fimple nature. All the chemifts of Europe feem to be agreed, that either 21 or 22 *per cent.* in bulk, is the proper number. My own experience gives 21 for the neareft integer, which agrees with Mr. Davy's in your Journal, (quarto Series), Vol. V. page 175 : if Mr. G. entertains any doubt on the fubjeft, I would refer him to that paper, and he will receive ample fatisfaction.

After what has been faid, I think Mr. G. muft be fatisfied that he has virtually demonftrated the atmofphere to be a mechanical mixture of the two gafes: If, however, he ftill alledge that I only oppofe one authority to another, and that his is as good as mine; then I would recommend him to fatisfy himfelf as follows: mix 21 parts (or a quantity of gas containing 21) of pure oxigen, and 79 of pure azote, together; after this proceed to the analyfis of the gafes, and examine the differences between the refults, and thofe done from the analyfis of 100 parts of atmofpheric air.

I cannot conclude this article without obferving, that Mr. G. muft have been totally unacquainted with the opinions of chemical philofophers on the fubject, or he would not have expanded into the compafs of five or fix pages a fimple argument, which has often been adverted to by others, and is now wholly abandoned as untenable. Mr. Davy, in his Refearches, publifhed in 1800, advocating the notion of atmofpherical air being a chemical compound, produces four evidences, one of which is ftated as follows : *See Page* 326.

" *2dly,* The difference between the fp. gr. of atmofpheric air, and a mixture of 27 parts oxigen and 73 nitrogen, as found by calculation; a difference apparently owing to expanfion in confequence of combination." In a note he adds, " The two firft evidences have been often noticed." This gentleman however foon after, finding that the atmofphere contained only 21 *per cent.* of oxigen, muft have feen that this evidence was not to be admitted. Since that time it has not been urged by any one to my knowledge.

I come next to Mr. G.'s reply at page 160. This at the commencement purports to be a defence of the charge, that *my arguments are but few, and negligently conducted;* at the conclufion it is afferted to be an *anfwer to all my objections, and fomething more.* This language may be that of logical

precifion

precifion with Mr. G.; but it would have been more intelligible to me if he had ftated the objects of his reply thus:

1ft, A refutation of Mr. D.'s arguments in favour of his fyftem.

2d, An anfwer to his objections brought againft mine.

3d, New objections to his fyftem.

The firft thing worthy of notice is the objection to my argument for the mutual penetrability of gafes. I have affumed one poftulate and taken two, and the latter of them is erroneous, namely, that all gafes are porous. It is true, I have taken two poftulates into the argument, without exprefsly requiring both; the former being peculiar to my theory, was neceffarily demanded in a formal way; the latter being the refult of all experience, and never in any one inftance having been found to fail, I thought it might tacitly be affumed. However, the judgment of philofophers muft be fufpended on this head, as Mr. G. it feems, is about to prove that no gas is porous, and that a cubic foot of one gas cannot be put into a veffel that is previoufly occupied by another gas. Mr. G. furely cannot be ferious in this objection; but merely ufes it to gain time, and means to turn it off with a laugh, that he has at leaft produced one folid argument againft my airy hypothefis.

Mr. G. finds it extremely convenient for his purpofe, that I fhould grant him the following poftulate: " If a particle of vapour can pafs freely through the air, a fecond can alfo fucceed it at any given diftance." I certainly cannot concede fo indefinite a demand; but it will perhaps be of equal ufe to him to have the following: If d be the diftance of two particles of vapour of the temperature of 212°, and preffure 30 inches; then, at the temperature of 60° or upwards, if one particle of vapour can pafs freely through the air, a fecond may fucceed it at any diftance greater than 4 d.

As for the important argument which I confider equivalent to a demonftration of the nature of vapour, and of its relation to gafes, and entirely inimical to the notion of chemical affinity, Mr. G. has not ventured to revive it: Probably he has fomething in referve on this head. I mean the argument derived from the fact, that a vacuum, or any kind of air of any denfity whatever, takes up juft the fame quantity of any vapour.

The

The propofition refpecting the mechanical action of air on the furface of water, will be anfwered by Mr. G. when he has proved that *air has no pores*, or no *capacity for the reception of water*. For, nothing can be more clear than that, whatever may be the preffure of the atmofphere, and however few the points of its action, water cannot be forced into the pores of air, if it have none.

Mr. G. proceeds to ftate two new facts, which are faid to be inexplicable on my principles: I muft undertake the arduous tafk. The facts are, 1ſt, Air containing aqueous vapour is fpecifically lighter than air without it, *cæteris paribus*; and, 2d, A bottle internally moift, containing air, being heated, more air is expelled than if the bottle had been dry. Both granted.—Now for the explication. The fpecific gravity of aqueous vapour has been found by De Sauffure, Watt, and others, to be about ⅔ or ¼ of that of atmofpheric air in like circumftances; by fome experiments of my own I am induced to think it is nearly .7, that of air being 1. Let the temperature be 64°, and the air be filled with vapour as much as poffible in the temperature, in which cafe $\frac{1}{50}$ of the elaftic force will be due to the vapour. (*See Manch. Mem. Vol. V. P. 2, page* 559); then, by the theorem fo elaborately exemplified in Mr. G.'s former letters, we have $\frac{49 \times 1 + 1 \times .7}{50} =$

$\frac{49.7}{50} = .994$, for the fpecific gravity of common air filled with vapour at 64°, when that of dry air of the fame temperature would be 1. Thus it appears that my hypothefis not only explains the fact of diminifhed fpecific gravity, but accounts for the quantity of diminution. Can Mr. G.'s theory of chemical folution do this?

In the fecond cafe we find heat generating vapour, which increafing in quantity and force with the temperature, diffufes itfelf through the air in the bottle; the hand being occafionally removed from the mouth, fuffered the extra-portion of air and vapour to ftart out; juft as if there had been a generation of a like portion of oxigen, or any other permanent gas, inftead of the vapour; in which cafe a portion of both does certainly efcape. Mr. G. finds the vapour generated this way between 59 and 126°, to be rather lefs than ⅕ of at-

mofpheric force; it ought to be juft ⅐ by the table above referred to. But it is obvious, that by removing the hand occafionally, the *full* quantity of vapour for any temperature could never be obtained in this way, no more than pure oxigen could be procured in the bottle by a fimilar procefs. The grand queftion with Mr. G. however, is, How does vapour of half an inch force, expand the pores of air fubject to 30 inches of preffure ? And his anfwer feems to be, it is impoffible, according to the axioms of dynamics. This queftion is what I fhall now confider.

Having myfelf ftudied the principles of dynamics, as well as thofe of many other mathematical and phyfical fciences, under the tuition of Mr. Gough, I feel under ftrong obligation to him ; but thefe, he will readily grant, do not bind me to fubfcribe to his opinions, when I cannot perceive them to be well founded. He charges me, in the prefent inftance, with a miftake in regard to dynamics; but he has not pointed out any particular axiom which I have offended : The miftake, I think, is with him, and fhall endeavour, in what follows, to point it out.

It is a principle in dynamics, that whenever a fyftem of bodies act upon each other, and are in a ftate of equilibrium, the *leaft* force impreffed upon any one difturbs the equilibrium. Thus, the ocean and the air, though bound to the earth by its fuperior attraction, are neverthelefs difturbed by the feebler influence of the moon. Air in a bottle is a fyftem of particles at equal diftances repelling each other, but in equilibrium by the gravity of the incumbent atmofphere ; confequently the *leaft* force impreffed upon them muft difturb that equilibrium. Now, *ex hypothefi*, air does not repel vapour at a diftance, but only in contact; therefore vapour can be formed in fuch a fyftem : when once formed, it meets with no elaftic refiftance or repulfion but from particles of its own kind; therefore it is conftantly tending outwards, where the particles of its own kind are leaft denfe : in its way it infringes upon particles of air, and exerts fuch force as it is capable of upon them : a number of particles of air are thus gently propelled in the direction of the ftream, and the reft of the fyftem are obliged to accommodate themfelves in order to preferve the equilibrium : thus the diftances of the particles of air are gra-

dually

dually increafed, and that in proportion to the force which the vapour exerts. Therefore vapour of the leaft poffible force can, in fuch circumftances, extend the pores of air. **Q. E. D.**

I remain your's,

J. DALTON.

Manchefter, Nov. 15, 1804.

XIV.

Some Account of a Condenfer of Forces, or a Method of obtaining the greateft poffible Effect from a firft Mover, of which the Energy is fubject to Increafe, or Diminution within certain Limits; and in general to vary at Pleafure the Refiftance to which the Effort of the firft Mover forms an Equilibrium in any Machine whatever, without changing any Part of the Conftruction. By R. PRONY.*

THE problem of mechanics, of which the folution is here given, is one of the fmall number of thofe which, leading to refults independent of the particular mechanifm of the machine to which they are applied, prefent, in their folution, a generality which may be compared with that of the rational mechanics, or analyfis. *Problems in mechanics,*

It may be enunciated in the following terms :

" Any machine being conftructed, to find, without making any change in the conftruction, a means of tranfmitting to it the action of the firft mover, by fulfilling the following conditions ; *viz.* *enunciated.*

" 1. That it may be poffible at pleafure, and with great fpeed and facility, to vary the refiftance (againft which the effort of the firft mover muft continually make an equilibrium) in limits of any required extent. *Conditions.*

" 2. That the refiftance being once regulated, fhall be rigoroufly conftant until the moment when it is thought proper to increafe or diminifh the fame.

" 3. That in the moft fudden variations of which the effort of the firft mover may be capable, the variation in velocity of the machine fhall never undergo a folution of continuity."

* From the Bulletin of the Philomathic Society at Paris, No. 83.

I fhall

I ſhall here apply the ſolution which I have diſcovered of this problem, to the dynamic effect of wind; it will be eaſy to make the ſame general when the other firſt movers are uſed.

The ſection and plan of the machine are exhibited in Plates XV. and XVI. O O repreſents the vertical arbor to which windmill ſails are adapted; *e e e e* is an aſſemblage of carpentry, of which one of the radii O *e*, bears a curved piece *b d*, of iron or ſteel: Vertical axes of rotation *a a a*, being placed round the axis O O, alſo divide the circumference in which they are found into equal parts.

Each of theſe axes carries a curve *a f*, of iron, ſteel, or copper; ſo ſituated, that when the wind acts upon the ſails, the curve *b d* preſſes againſt one of the curves *a f*, and cauſes the vertical axis to which this laſt curve is fixed, to make a portion of a revolution.

The curves *b d* and *a f* muſt be ſo diſpoſed, that when *b d* ceaſes to preſs on one of the curves *a f*, it ſhall at the ſame inſtant begin to act upon the following curve: the number of axes which are provided with theſe curves, muſt be determined by the particular circumſtances of each caſe; and it is alſo practicable to ſubſtitute, inſtead of *b d*, a portion of a toothed wheel having its centre in the axis O O, and to place portions of pinions inſtead of the curves *a f*, but the diſpoſitions repreſented in the figure are preferable.

Each of the axes *a a a a* (which are all fitted up alike, though, for the ſake of clearneſs, only one of them has its apparatus repreſented in the drawing), each of theſe carries a drum or pulley *t t r r*, on which is wound a cord that paſſes over a pulley *p*, and ſerves to ſupport a weight Q by means of the lever F G, upon which this weight may be ſlided and faſtened at diffeſent diſtances from the point of motion G.

The ſame axes *a a* paſs through the pinions *q q*, to which they are not fixed; but theſe pinions carry clicks or ratchetts, which bear againſt the teeth *r r*; ſo that, when the weight Q tends to riſe, the ratchett gives way, and no other effect is produced on the pinion *q q*, either by the motion of the axis or of the drum *t t r r*, excepting that which cauſes the aſcent of the weight *q q*. But the inſtant that the curve or tooth *b d* ceaſes to bear againſt one of the curves *a f*, after having cauſed the correſponding weight Q to riſe, that weight Q tends to redeſcend, and then the toothed wheel *r r* acts againſt

the

the ratchett, fo that Q cannot defcend without turning the pinion qq along with the drum $t\,t\,r\,r$.

The pinion qq takes in the wheel ab, from the motion of which the ufeful effect of the machine immediately refults; fo that the effect of the defcent of one of the weights Q, is to folicit the wheel A B to motion, or to continue the motion in concurrence with all the other weights Q, which defcend at the fame time. This wheel A B carries beneath it oblique or bevelled teeth G D, which take in a like wheel C E, and caufe the buckets at S to rife.

The alternation in the motion of thefe buckets may be effected by the mechanifm I have defcribed in the firft volume of the Memoirs of the Inftitute.

From the preceding defcription it is feen that the machine, being fuppofed to ftart from a ftate of repofe, the wind will at firft raife a number of weights Q, fufficient to put the machine into motion, and will continue to raife new weights while thofe before raifed are fallen; fo that the motion once impreffed will be continued.

Among the numerous advantages of this new mechanifm we may remark the following :

1. No violent fhock can take place in any part of the mechanifm.

2. The ufeful effect being proportioned to the number of weights Q, which defcend at the fame time, this effect will increafe in proportion as the wind becomes ftronger, and caufes the fails to turn with more velocity.

3. The weights Q being moveable along the levers F G, it will always be very eafy to place them in fuch a manner as to obtain that ratio of the effort of the firft mover to the refiftance, as fhall produce the maximum of effect.

4. From this property it refults, that advantage may be taken of the weakeft breezes of wind, and to obtain a certain product in circumftances under which all other windmills are in a ftate of abfolute inactivity : this advantage is of great importance, particularly with regard to agriculture : the windmills employed for watering lands are fometimes inactive for feveral days, and this inconvenience is more particularly felt in times of drought. A machine capable of moving with the flighteft breeze, muft therefore offer the moft valuable advantages.

I fhall

I ſhall give a more ample account of this apparatus in a memoir which I ſhall preſent to the Inſtitute, as ſoon as the machine which I am now erecting in the country ſhall be completed. *

XV.

Abſtract of a Memoir on the Poſſibility of obtaining Pruſſiate of Potaſh free from Iron; the Unalterability of the Pruſſic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Baſes.† By BUCHOLZ.

Utility and advantages of pruſſic compounds.

THE combination of pruſſic acid with ſalefiable baſes, forms a claſs of ſalts of the greateſt utility to the analytical chemiſt; for by means of them he is not only enabled to aſcertain whether a metallic ſubſtance be preſent in any ſolution which forms the ſubject of his reſearch, but alſo what metal is preſent, as well as its reſpective quantity. But in order to be accurate in this reſpect, the re-agent employed muſt itſelf be free from metallic admixture; or the quantity and nature of the metal it contains muſt at leaſt be known. To accompliſh this, chemiſts have hitherto laboured in vain. To remedy theſe defects, Mr. Bucholz has inſtituted a number of experiments which led to facts hitherto unknown; and as they are highly important, we ſhall exhibit the reſults of the principal ones, which are as follows:

* The practical mechanic may perhaps find it an advantage to be informed, that the whole effort of a firſt mover cannot be tranſmitted by this, or any other method of raiſing weights, in order to operate by their fall. If the wind had been employed to raiſe a maximum of weight through a given ſpace in a given time, this weight would be leſs than would continue in equilibrio at reſt againſt the ſame force, as is well known; and if this weight be again employed in like manner to raiſe another maximum of weight, this alſo will be leſs than the former, &c. For this reaſon it is that the fly has been uſed as an equalizer of ſteam-engines, in preference to the older method of raiſing water and ſuffering it to deſcend on an overſhot wheel.   W. N.

† Abridged from a memoir in Gehlen's new Journal of Chemiſtry, Vol. I. Part IV. Page 406, by — A.

1. Pruſſic

1. Pruffic acid can only be formed during the carboniza-tion of blood or animal matter, *at a red heat:* We, therefore, need not be afraid of heating the mixture of blood and alkali, in the preparation of pruffiated alkalies, to incandefcence.

Pruffic acid is formed only at red heat.

2. The affinity of pruffic acid with alkalies is greater at high temperatures, and in the *dry way,* than at low tempera-tures, and in the *humid way.*

Its affinity with alkalies is then greateft.

This obfervation diftinguifhes pruffic acid from the reft of the fo called animal and vegetable acids, and their combina-tions with alkaline bafes; for all of them are deftroyed at a red heat. Hence it is obvious,

All other vege-table acids are deftroyed by ig-nition.

3. That in the preparation of pruffiated alkalies, the pre-fence of water fhould be avoided as much as poffible.

Water fhould be avoided.

4. The direct combination of pruffic acid with alkalies, can-not be accomplifhed.

P. acid and alk. do not directly combine.

5. Pure pruffiated alkalies are decompofable by the affufion of water; part of the pruffic acid efcapes, and may eafily be recognifed by the odour of bitter almonds, and frequently alfo by that of ammonia.

P. pruffiated alk. decompofable by water.

6. The combination of pruffic acid with alkalies can only be effected *at all temperatures* up to a red-heat, by the interpofi-tion of a portion of oxide of iron; and the affinities confifting between the pruffic acid and alkali, are retained with a greater force in the humid way, in the ratio of the ponderable quan-tity of iron prefent.

P. acid does not combine with alk. at low temp. but by medium of iron, &c.

7. All the precipitates obtained in chemical analyfis by means of pruffiated alkalies prepared in the ufual man-ner, contain more or lefs iron, without exception; whereas the fame precipitates, produced by the action of abfolutely pure pruffiates, are free from that metal, and of a different colour than the former.

Common pruf-fiates contami-nate metals with iron; pure p. give a different refult.

8. The affinity of oxide of iron to charcoal, is more power-ful than the joint attraction of pruffic acid and potafh to that metal; hence we always find that the greateft quantity of that metal remains behind with the charcoal, in the ufual procefs for obtaining pruffiate of potafh.

Moft of the iron prefent re-mains in the coal, when p. alk. is made.

9. The precipitability of the oxides of metals by pruffiated alkalies, is in the ratio of their oxidability, or quantity of oxigen they contain.

Metals are pre-cip by p. alk. in the order of oxi-genation.

Such are the obfervations of this chemift. Other lefs in-terefting facts will be omitted in the prefent abftract.

Experimental

*Experimental Enquiries concerning the beſt Method of preparing
Pruſſiate of Potaſh free from Iron.*

Preparation of
pruſſiate of al-
kali, pure.

Low ignition of
dried blood and
potaſh, and ſo-
lution by water.

Four oz. dried blood were intimately mixt with a ſolution
of potaſh (containing one ounce of potaſh), then evaporated
to dryneſs, ignited to redneſs, till no more flame undulated
at the ſurface of the ignited maſs. The ignited maſs was
diffuſed through ſix ounces of water, and the ſolution filtered.
The filtered fluid was colourleſs, it contained an exceſs of
alkali, and emitted a ſtrong odour of bitter almonds. It
yielded, on being evaporated as expeditiouſly as poſſible, a
ſaline maſs, confiſting of a mixture of pruſſiate and ſub-car-
bonate of potaſh. In order to ſeparate theſe two ſalts, one
drachm of the ſaline maſs was introduced into a vial con-
taining a mixture of half an ounce of highly concentrated

The ſalt was
treated with ace-
tic acid and alco-
hol.

alcohol and half a drachm of acetic acid, of 1,056 ſpec. grav.
On agitating the fluid no fenſible efferveſcence took place,
but much pruſſic acid was difengaged; a proof the pruſſic
acid was retained with a leſs affinity by the alkali than the
carbonic acid. On examining the reſidue, which had been
treated with acetic acid and alcohol, it was found to contain
only a ſmall portion of pruſſiated alkali.

Very little pruſ-
ſiate, and not
ſeparable from
the carbonate by
thoſe agents.

From this experiment we learn, that one part of potaſh
cannot be converted into pruſſiate of potaſh, by being heated
to redneſs with four of blood; and that the quantity of either
free or carbonated alkali, under theſe circumſtances, can-
not be ſeparated from the pruſſiated potaſh by means of
acetic acid and alcohol.

Leſs of blood
was then uſed
in the ignition.

In order to learn if a leſs quantity of blood would not be
more advantageous for the production of pure pruſſiate of
potaſh, four ounces of dried blood, and two of carbonate of
potaſh, were heated to redneſs in a crucible till no more flame
appeared. On covering the mixture with charcoal powder,

Great evolution
of ammonia.

and again heating it, a prodigious evolution of ammonia took
place; a phenomenon I do not venture to explain. The
maſs, after having been diffuſed through water, filtered and
evaporated, yielded a crop of cryſtals, confiſting of pruſſiate
and ſub-carbonate of potaſh, the former predominating con-
ſiderably.

The ſubcarbon-
ate and pruſſiate
not ſeparable by
alcohol and
acetic acid.

On ſubjecting this mixture of ſalts to the joint action of
acetic acid and alcohol, it was found impoſſible to ſeparate
the pruſſiate of potaſh from the ſub-carbonate.

Being

Being thus perfuaded that a lefs quantity of blood was not more advantageous for forming pure pruffiate of potafh, I again mingled two ounces of dried blood with one of carbonate of potafh: this mixture was treated as before, with the exception that the mafs, after the flame had entirely difappeared, was ftrongly ignited for three quarters of an hour. The obtained mafs, after refrigeration, weighed nine drachms. Digefted cold with four ounces of water, and filtered and evaporated, it afforded *a dark coloured* fluid. On dropping into a confiderable quantity of muriatic acid, no blue, but a white precipitate fell down, which was infoluble in muriatic acid: Acetic acid occafioned no change in this dark-coloured fluid; and on mingling it with fulphuric acid, and evaporating the mixture to drynefs, and re-diffolving the mafs in water, it yielded a fmall quantity of pruffian blue; a proof that iron was prefent in this fluid.

Blood and alkali, as in the fitft experiment, ftrongly ignited tor three quarters of an hour.

The fluid of folution was dark, and contained fome iron, though very little.

The quantity of iron being very fmall, but the colour of the fluid very dark when compared with thofe of the former proceffes, it was natural to fuppofe, that the colouring-matter could not be attributed to the minute quantity of iron prefent, but that perhaps a portion of charcoal was diffolved in the fluid: to inveftigate which the following experiments were inftituted:

A like quantity and like proportions of blood and carbonate of potafh, as ftated laft, were gradually heated to incandef-cence, and the fire gradually augmented, *until the mafs began to fufe* on the fides of the crucible. The mafs, after having been diffufed through water and filtered, yielded a much darker-coloured fluid, which, when mingled with muriatic acid, yielded a pearl-coloured precipitate. After being mixt with muriatic or fulphuric acid, evaporated and rediffolved in water, it afforded a confiderable quantity of oxide of iron; a proof that the union of the pruffic acid with potafh is permanent at very high temperatures, but that this combination, under fuch circumftances, exercifes a ftrong action upon the oxide of iron contained in the blood.

Repetition of the experiment with fufion of the mafs.

The pruffic acid remains united with alk. at very high temp. and takes iron ftrongly. Lefs oxide taken up by lefs heat.

Thefe facts were proved by repeating this experiment, but taking care to expofe the mafs to a fomewhat lefs degree of heat. The fluid now obtained was lefs coloured, and yielded lefs oxide of iron.

This

Experiments with pure alkali.

This being proved, the author varied his experiments, fo as to be thoroughly convinced of the facts. He also endeavoured to afcertain, whether alkalies, freed from carbonic acid, were better calculated for the production of pruffiated alkali; and, if poffible, to find out the proper proportions of ingredients for obtaining this falt. With that view, a quantity of folution of potafh, containing one ounce of alkali freed from carbonic acid, was mixt with three ounces of blood, and evaporated to drynefs. (It was of a colophony colour, foluble in water, and emitted, on being heated, a very ftrong odour of ammonia). On being transferred into a crucible,

Low heat produced little combination of p. acid and the alkali.

and gradually heated till no more flame appeared, and diffufed through four ounces of water, it yielded a *limpid* fluid, of a ftrong alkaline tafte, and odour of bitter almonds. The ufual experiments proved, that it contained comparatively little pruffiated alkali, but a large quantity of carbonate of potafh.

Exp. to determine the proportions of carbonate and blood requifite to produce the pruffiate, &c.

Being thus convinced that this experiment proved fruitlefs, it was deemed neceffary to afcertain the refpective quantity of oxide of iron contained in the pruffiated alkali, that might be produced from a given quantity of blood and carbonate of potafh.

The experiments with low heat.

4 oz. blood and 1 alk. carb.

With that view, four ounces of dried blood were mixt with a folution of carbonate of potafh, containing one ounce of carbonated alkali: the mixture, after being evaporated to drynefs, was heated till the flame ceafed to appear. It now, weighed 1¼ oz. On being elixiviated with eight ounces of water, it afforded a fluid of a very pale yellowifh brown, or wine colour: Its tafte was alkaline, mixt with that of bitter almonds. Two drachms of water mixt with 20 drops of a concentrated folution of muriate of iron, when decompofed

Product very little.

by this pruffiated alkali, afforded four grains of pruffian blue, ftript from the adhering oxide of iron by muriatic acid.

2 oz. blood, 1 carb. alk. low heat.

Product not increafed.

Two ounces of dried blood and one of carbonate of potafn, treated in a fimilar manner, yielded, by being mingled with a like quantity of muriate of iron, 4½ grains of pure pruffian blue.

(The Remainder in our next.)

Obfervations

XVI.

Obfervations on the Caufe which augments the Intenfity of the Sound in Speaking Trumpets. By J. H. HASSENFRATZ *.

ALTHOUGH the fpeaking-trumpet is an inftrument which has been long known, fince Kircher is of opinion that Alexander ufed it to command his army, and Solard made one in Paris, in 1654, from the defcription which Kircher had given of that of Alexander, it was not, however, until 1671, when Morland made known that which he had conftructed, and invited men of fcience to determine the figure moft proper for this inftrument, that the fpeaking-trumpet was really known among us, and began to be ufed.

It appears that Morland did not adopt the conical form terminated by a mouth piece, which he gave to the fpeaking trumpets of glafs, iron and copper, he made until after a fucceffion of trials on the beft form to be given to them, to make them produce articulate founds.

It alfo appears that it was without knowing it, and by chance, that Caffegrain gave the fpeaking trumpet, which he made in 1672, the hyperboloidal form firft noticed by Sturm.

Until 1719, when Haffe publifhed a differtation on the improvement of fpeaking trumpets, this inftrument was conftructed without principles; for we neither can nor ought to confider as principle, the harmonic proportion mentioned by Caffegrain as being neceffary between the length and the width of the tubes of fpeaking trumpets.

The law of the reflection of found in echoes, led Haffe to apply the theory of catoptrics to fpeaking trumpets, and induced him to confider the combination of the *ellipfoidal* and *paraboloidal* forms as the moft advantageous for this inftrument: but this union not producing the effect which the profeffor of Wirtemberg had hoped for, it was abandoned, and in conformity with his opinion, the augmentation in the intenfity of the found continued to be confidered as the product of the reflection of the fonorous rays in the fpeaking trumpet, and of the vibration of the fubftance of which it was compofed. It was for this reafon that, in the fabrication of thefe inftruments, the

Marginal notes:
Antiquity of the fpeaking trumpet.

Morland's.

Caffegrain's.

Were at firft conftructed without known principles.

Haffe's theory of the augmentation of the intenfity of found in them.

attention

attention was directed to having a hard, elaftic and thin fub-
ftance, that it might vibrate in unifon with all the tones, and
reflect the fonorous rays which ftruck it in all directions.

In 1763, Lambert withdrew from the theory of fpeaking
trumpets, the vibration of the materials of which they are com-
pofed: he fhowed that the vibration of the fides which is cal-
culated to augment the intenfity of a long continued found,
would render articulate founds confufed, which muft fucceed
each other rapidly ; that in this cafe therefore it would be ne-
ceffary to fpeak extremely flôw, and that, even by fpeaking
flowly, it would be impoffible to diftinguifh the confonants,
which are only momentary modifications of the vowels; that
the latter, pronounced in the trumpet, would be fo fonorous,
that the confonants muft be gueffed at, which would be ex-

Speaking trum-
pets do vibrate,
but the found is
equally ftrong
when the vibra-
tion is prevented.

tremely difficult. By fprinkling the exterior furface of a tin
fpeaking-trumpet with faw-duft, I fatisfied myfelf that the
furface vibrated in fome circumftances, but I was equally fa-
tisfied, on covering the outfide of the trumpet with a foft loofe
fluff to ftop and obftruct its vibrations, that the intenfity of
the found was no lefs ftrong, in this fecond cafe, and, that
thus the vibration of the fubftance of the trumpet was, at leaft,
ufelefs, if it was not injurious to the diftinctnefs of articulated
founds.

In his memoir, publifhed among thofe of the Academy of
Berlin, for the year 1763, Lambert has attributed all the aug-
mentation of the intenfity which the found experiences by
fpeaking into a trumpet, to the reflection of the fonorous rays
on the fmooth and polifhed furface of the interior of this inftru-
ment; he fays that, of all the forms, that beft calculated to
concentrate the found by reflecting it, and the moft eafy to

conftruct, is the conic form: he afterwards fhows that the
found is ftrengthened in conical fpeaking trumpets, in the pro-
portion of double the length of the cone which forms the trum-
pet to double the fize of half the angle at the fummit of the
cone, fuppofing the length of the cone to be equal to the ra-
dius. If the angle of the cone is made $= \phi$, the found is
ftrengthened in the proportion $\sqrt{2} : 2$ (fin. $\frac{1}{2}\phi$).

Seeking, from thefe formulæ, to difcover what would be
the beft proportions to give to conical fpeaking trumpets, he
found that whatever the angle of the fummit of the cone was,
its bafe muft be equal to the diftance between the fummit of
the

the cone and the mouth of it, and that its length muſt be equal to the diameter of the mouth, divided by four times the ſquare of the ſine of half the angle of the cone : that thus in a trum- pet of ſix feet in length, the angle of the cone muſt be 16° 17, and that ſuch a ſpeaking-trumpet would make the ſound 96 times more intenſe.

Lambert therefore, as well as thoſe who have preceded him, confidered the augmentation of the intenſity of the ſound in ſpeaking trumpets, as the reſult of the action of the reflection of the ſonorous rays againſt the interior ſides of the inſtrument.

It would appear that ſince Lambert's memoir, ſpeaking trumpets have been made uſe of, without attending to the cauſes which augment the intenſity of the ſound, and, in fact, it is ſeen that in all works in which this inſtrument is ſpoken of, its effects are attributed either to reflection alone, or to reflec- tion combined with the vibration of the ſubſtance of which it is formed. *Theſe inſtru- ments have lat- terly been con- ſtructed without attending to the cauſes of the increaſe of ſound.*

If we compare the theory of ſpeaking trumpets with that of the inſtruments which have ſome reſemblance to it, ſuch as trumpets and hunting horns, we muſt be aſtoniſhed to find that they are referred to different principles. In the ſpeaking trum- pet, the cauſe is attributed to the reflection of the ſound; in the other inſtruments to the vibration of the air contained in the tube. Why theſe two cauſes, while the effects are analogous? This analogy has led me to inquire if, in reality, the reflection produced the augmentation of the intenſity of articulation in horns and acouſtic tubes, as well as in ſpeaking-trumpets, as is generally believed. *Theory of ſpeak- ing trumpets and other inſtru- ments.*

Does reflection increaſe the ſtrength of ſound ?

On examining the ears of animals, it is ſeen that the great- eſt number have an exterior auricle, which moſt of them di- rect towards the place from which the ſound proceeds; this auricle being, in many animals, covered inwardly with hair, which ſtops and hinders the reflection of the ſound, it is rea- ſonable to conclude that it is not by reflection that the ſound is tranſmitted into the ear. *Animals do not hear by the re- flection of ſound.*

When afterwards we obſerve the form given to acouſtic horns, which is that of a cone, the ſummit of which, ſlightly trun- cated, is placed in the ear, we are quickly led to conclude that the ſound is not tranſmitted by reflection; for the angle of the incident ray being increaſed at each reflection by that of the cone, after a number of determinate reflections, the angle would *Ear-trumpets do not tranſ- mit ſound by reflection.*

would become larger than a right angle, and the ray would return upon itfelf. Thus the greateft number of the rays would iffue out of the mouth, after being feveral times reflected, and the quantity of the rays which thus reach the ear, and which are proportionalby lefs numerous as the cone is longer, cannot fenfibly augment the intenfity of the found. Neverthelefs, acouftic horns do ftrengthen the found confiderably; this augmentation muft therefore depend on another caufe.

<div style="margin-left:2em">**Horns of a paraboloid form difufed.**</div>

It has been propofed to make horns of a paraboloidal form, as being beft calculated to tranfmit found by reflection; but whether they did not produce the effect which was expected from them, or that they were too difficult to conftruct, thefe horns have been abandoned, and none but conic horns are made ufe of.

<div style="margin-left:2em">**Cylindrical acouftic tubes tranfmit found without reflection to a great diftance.**</div>

The acouftic tubes employed to tranfmit the found to great diftances, fo that it cannot be heard by perfons placed between the two extreme points, are ufually formed of cylinders, which ferve to conduct the found. Lambert had already ftated that reflection did not increafe the intenfity of found in cylinders, becaufe whatever might be the direction of the incident rays, being conftantly reflected between two parallels, the angle which they form with the axis of the cylinder muft be the fame at entering and quitting it; that thus the found muft experience a diminution at its iffue, which will be fo much the greater as the number of reflections is more confiderable. Neverthelefs the found is tranfmitted to a great diftance by means of thefe tubes, and I have afcertained that the beating of my watch, which ceafes to be audible at the diftance of 1ᵐ. 10, the medium of feven experiments (A), is heard at the diftance of 2ᵐ 25, the mean diftance of feven experiments (A) when I place it in the mouth of a pafteboard tube, 0ᵐ. 038 in diameter and 0ᵐ. 6 in length; whence it follows that this tranfmiffion muft proceed from another caufe.

<div style="margin-left:2em">**According to the theory of reflection the enlarged part (pavillion) is of no ufe.**</div>

All thefe confiderations have therefore induced me to examine the effects of the fpeaking trumpet with the greateft care. It follows from the theory of the reflection of found, applied by Lambert to the fpeaking trumpet, that the opening by which it is ufually terminated, fhould be at leaft ufelefs, if not injurious, for it has no influence on the concentration of the reflected rays; and therefore this gentleman propofed to lay it afide entirely. The analogy of form between the trumpet

pet

·pet and the fpeaking trumpet not being a fufficient reafon for retaining it, it is probable that it would have been laid afide, in the fame manner as the contour of the trumpet was abandoned, when the form and dimenfion? moſt fuitable to this inſtrument were fought for by experimental trials. There would alfo have been another inducement to difpenfe with it, becaufe Haffe had already omitted it in the fpeaking trumpet which he indicated as preferable to thofe of Morland and Caffegrain. I afcertained by experiment that two fpeaking trumpets of the fame length and the fame diameter produced different effect, when they were terminated by an enlarged aperture or not, and that in general this enlargement confiderably increafed the intenfity of the found. The beating of my watch which I heard at 4ᵐ. 2, the mean diſtance of feven experiments *(a)* by placing it at the mouth of a fpeaking trumpet of 0ᵐ. 6 long and 0ᵐ. 38, mean diameter of the tube, was not heard beyond 2ᵐ. 25, mean diſtance of feven experiments *(a)* when I placed it in a tube of the fame length and the fame diameter. Thus the caufe which occafions this aperture to ſtrengthen the fooner is different from the reflection of the fonorous rays.

Since the reflection of found does not in any manner concentrate the fonorous rays in a cylindrical tube, it muſt follow, as Lambert has concluded in his Theory of Reflection, that fpeaking trumpets with cylindrical tubes fhould not fenfibly augment the found. To prove whether experiment agreed with this theory, I immediately conſtructed a fpeaking trumpet, with a cylindrical tube of 0ᵐ. 36, the length of the cylinder, 0ᵐ. 25, the length of the enlarged part, 0ᵐ. 035, the diameter of the cylinder, and 0ᵐ. 190, the greateſt diameter of the enlarged part, and, by comparing the intenfity of the found which it produced, with that of a conical fpeaking trumpet of the fame height, and of a fimilar mean diameter, I afcertained that the ſtrength of the found was fenfibly the fame. In both, the beating of my watch, which I did not hear in the open air beyond 1ᵐ. 08, mean diſtance of five experiments *(b)* was heard at 5ᵐ. 94, mean diſtance of five experiments *(b)* when I placed it at the mouth of either fpeaking trumpet. Since the cylindrical fpeaking trumpet ſtrengthens the found in the fame manner as that which is conical, and fince, on the contrary, the theory of reflection fhows that it fhould not be ſtrengthened, it follows that the augmentation of found in thefe inſtruments arifes from a caufe different from reflection.

One

One decifive experiment remained to be made with thefe inftruments, this was to deftroy and to reftore the action of the reflection in a fpeaking trumpet, fuppofing it to take place, and to compare the intenfity of the found obtained in thefe two circumftances, in order to determine the proportion of the effect produced by this reflection. I confequently procured a fpeaking trumpet of woollen ftuff, which I placed within one of tin, and I obferved that the intenfity of the found obtained with or without the ftuff in the infide, was fenfibly the fame; whence it follows that the reflection has no appreciable influence on the augmentation of found in a fpeaking trumpet.

I have faid that the intenfity of the found was fenfibly the fame; neverthelefs I muft obferve that it was diminifhed; for the beating of my watch, placed in the mouth of the bare fpeaking trumpet was heard at a diftance of 3^m. 94, mean of five experiments (b), while when placed in the mouth of the fame inftrument covered in the infide with the ftuff, it was not heard beyond 2^m. 48. This difference, which at the moment might be fuppofed to arife from the reflection of the found taking place in the one cafe, and not in the other, feems to depend folely on the diminution of the diameter of the cylinder of the fpeaking trumpet and of the enlarged parts occafioned by the ftuff placed within them: I fatisfied myfelf that the fpeaking-trumpet which I made ufe of (b), preferving all its dimenfions and experiencing no difference except in the diameter of the tube, produced a diminution in the intenfity of the found, whenever a fmaller diameter was made ufe of; thus, in the fpeaking trumpet already mentioned (b), the beating of my watch was heard at a diftance of 3^m. 94, when the diameter of the tube was 0^m. 035; it was not heard beyond 3^m. 08, when the tube was only 0^m. 032; finally, it was not heard beyond 2^m. 34, when the tube was 0^m. 028. It is evident, therefore from thefe experiments, that the diminution in the intenfity of the found by covering the interior of a fpeaking trumpet with ftuffs, arifes from the diminution of the diameter of its tube.

I fhould obferve that if the internal ftuff occafions an inconfiderable diminution in the intenfity of the found, it, on the other hand, produces a great advantage by rendering the articulated founds clearer and lefs confufed.

It follows from all which has been fhewn on the auricles of animals, on horns, and acouftic tubes, and on fpeaking trumpets,

|pets, that the augmentation of found, in every circumftance, is not owing to the reflection of fonorous rays, confequently there exifts no reafon for feparating the caufe which augments the found in thefe inftruments, from that which ftrengthens it in trumpets and hunting horns; that the different founds produced in the two cafes are owing to the vibration of the air in the tubes, and their ftrengths or their intenfities, to the augmentation of the amplitude of their vibration, arifing from the greater impulfe which the air neceffarily receives when it is enclofed in a tube.

Experiment (A).

The tubes and the fpeaking trumpet of pafteboard.

The interior diameter of the tube of the fpeaking trumpet is 0^m. 038, that of the exterior diameter of the enlarged part $=$ 0^m. 210. The length of the tube $= 0^m$. 4, and the length of the enlarged part $= 0^m$. 2.

Diftance at which the beating of the watch was heard.	With a tube of the length of			The fpeaking trumpet.	Without tube or fpeaking trumpet	Obfervations.
	m. 0.4	m. 0.6	m. 0.10			The great difference between thefe experiments arifes, 1ft, from the difpofition of the organ jem ad, from the greater or lefs noife produced in the neighbourhood of the place where the experiments were made.
	1.33	1.66	2.33	4.00	1.00	
	2.24	2.90	3.33	4.66	1.16	
	2.16	2.66	3.33	5.00	1.33	
	1.33	1.79	1.85	3.00	0.83	
	1.92	1.95	2.00	5.00	1.33	
	1.58	2.16	2.66	3.66	1.00	
	2.54	2.66	3.17	4.00	1.00	
	12.86	15.78	18.67	29.33	7.65	
Mean	1.83	2.25	2.67	4.20	1.10	

Experiment (B).

The fpeaking trumpet was of tin, as well as the tubes.

The interior diameter of the tube of the fpeaking trumpet $= 0^m$. 035, that of the exterior diameter of the enlarged part $= 0^m$. 290, the length of the tube $= 0^m$. 36, and that of the enlarged part $= 0.25$.

	With an internal diameter of			Without speaking trumpet or tube.
	m. 0.035	m. 0.032	m. 0.028	
Distance at which the beating of the watch was heard.	4.88	3.58	2.76	m. 1.48
	3.58	2.93	2.28	0.97
	3.25	2.76	2.28	1.30
	4.39	3.58	2.44	0.82
	3.58	2.58	1.95	0.82
Sum	19.78	15.43	11.71	5.39
Mean	3.94	3.08	2.34	1.08

XVII.

*Account of Cerium, a new Metal found in a mineral Substance
from Bastnas, in Sweden. By W. D'Hesinger and
J. B. Bergelius [*].*

I.

Description of the Tungstein of Bastnas.

Discovery of
new substance
in tungstein.

ALTHOUGH this substance has been formerly assayed by
Scheele and D'Elhuyar, under the name of wolfram, its con-
siderable weight, nevertheless, determined us to submit it to
farther enquiries. Our principal object was to find yttria in
it, which, being unknown at the time in which these chemists
operated, might have escaped their attention. Our suppofi-
tion was not well founded, since, instead of an earth, we
discovered a substance which, according to every appearance,
is hitherto unknown, as will be seen in the sequel.

Where found.

The tungstein of Bastnas, which we call *cerite*, for reasons
which will be presently given, was found, in the year 1750,
in a copper-mine called Bastnas, or Saint-Gorans Koppar-
grufva, at Riddare-Hyltan, in Westmannia, of which, with
asbestos, it formed the matrix: but after this time it was im-
bedded in quartz and mica, to the depth of seventeen toises.

[*] From the Swedish, by G. A. Linborn; but here translated from
the Annales de Chimie, L. 145.

The

The tungftein is almoft always mechanically mixed with black amphibole *(hornblende)*, ftriated actinote, of a clear green colour *(fhorl)*, mica, fulphurated copper, bifmuth, and fulphurated molybdena, which may be eafily known by expofing it to the fire.

The cerite, properly fo called, is tranfparent, of a flefh colour, fometimes deep and fometimes bright, feldom yellowifh. In a mafs, or in fmall fpecimens, the ftone is of an irregular form; its fracture is indeterminate, compact, and a little brilliant, with obtufe edges; its confiftence is tenacious and ftrong; it gives fire with fteel with difficulty, but does not fcratch glafs; it is not attracted by the magnet; after having been made red-hot in the fire, it lofes its hardnefs, and fix or feven *per cent.* of its weight: by this operation it becomes friable, and acquires a bright yellow colour: it does not melt alone.

On account of its weight, Cronftedt has placed it among the tungfteins, in his Mineralogy. In pure fragments, it is to that of water as 4.733 and 4.935 to 1.000. Scheele not having found wolfram in it, called it falfe tungftein.

The conftituent principles of this mineral were given by Bergman, in the Memoirs of the Academy for the year 1784, page 121, from an analyfis of D'Elhuyar: they are as follows:—

Silex,	-	- -	0.22
Iron,	-	- -	0.24
Lime,	-	- -	0.54
			1.00

Heated with borax by the blow-pipe, it forms a globule of glafs, which, while hot, appears greenifh, but is colourlefs when cold. Urged with carbonate of foda in a platina fpoon it is not diffolved.

§ II.

Analyfis of the Proportions of Cerium.

TO feparate the yttria which was fuppofed to be in it, it was reduced to a fine powder in a porphyry mortar; pure concentrated nitric acid was then poured upon it. The acid was decompofed, and a confiderable quantity of nitrous gas

and

and carbonic acid gas were difengaged. The ftony powder was repeatedly treated with this acid, until the infoluble refidue appeared white.

The folution diluted with water was of a yellow colour, which became greenifh by boiling, and afterwards red : completely dried, it became of a yellowifh white, but regained its red colour by attracting humidity. It is entirely diffolved in alcohol ; and the folution, flightly digefted, depofits a confiderable quantity of oxide of iron. It likewife depofits more oxide of iron by remaining for fome days undifturbed in a window. The decanted folution, being almoft clear, was evaporated to ficcity, and the calcined falt was in the form of a powder, of the colour of bricks. Water could only diffolve the calcareous earth. Diftilled vinegar could only take up a very fmall portion, and was not faturated, though affifted by the heat of ebullition. The evaporated acetic folution gave fmall granulated cryftals, of a faccharine aftringent tafte. They were not totally foluble in alcohol. The part of the acetous falt which was not diffolved in alcohol, gave, by calcination, a brick-coloured powder, refembling that which had not been diffolved.

Ammonia precipitated the alcoholic folution in a white powder, which became yellowifh in the air. It was a little foluble by carbonate of ammonia, and acquired the colour of bricks by calcination. The fediment being feparated, the carbonate of ammonia produced a white precipitate, which was pure carbonate of lime. The acetous falt did not therefore contain yttria. The powder from which the calcareous earth had been feparated, diffolved in muriatic acid, with a difengagement of oxigenated muriatic acid gas, which indicated that there was a metallic oxide.

Indications of a metallic fubftance.

Was it oxide of manganefe combined with oxide of iron ?

To afcertain this, we endeavoured to develope the pure oxide of manganefe by means of tartrite of potafh, according to Richter's method. We decompofed in this manner, a folution of this fubftance in muriatic acid, perfectly neutralized by tartrite of potafh ; and after having wafhed the precipitate well, we fubmitted it to a flow calcination ; but it only produced the brick-coloured powder.

Does not contain alumine.

Cauftic alkali had no action on the infoluble part of the nitrate ; which proves that it did not contain alumine.

To

To obtain the pure metallic matter in a sufficient quantity to make several assays, another portion of cerite was dissolved in nitric acid, and the solution evaporated to siccity. Water was poured on the residue, and it was precipitated with ammonia. The washed precipitate was dissolved in nitric acid. The solution, well neutralized with the alkali, was afterwards precipitated by tartrite of potash. A white powder was also precipitated from the same solution by carbonate of potash, but it was in small quantity. These precipitates were separately calcined, and both of them acquired the colour of bricks. The precipitate formed by the carbonate of potash, was not dissolved by potash aided by digestion; it therefore did not contain alumine. The iron contained in the solution, precipitated with tartrite of potash, was separated by hidro-sulphuret of ammonia. The remainder of the solution of cerite in nitric acid, which had been precipitated by caustic ammonia, gave carbonate of lime by carbonate of ammonia.

From these assays it results, that cerite contains nearly 23 parts of silex, 5.5 of carbonate of lime, 22 of oxide of iron, and a quantity of this metallic matter, the weight of which, after calcination, rather exceeded 50 *per cent.* But this substance being then, as well as the iron, united with more oxigen than they contained in the cerite, we have, instead of a loss, an augmentation of weight, which probably arises from the oxigen. Neither is the loss which the cerite experienced in the calcination, included in this account. We also found traces of manganese, but in so small a quantity, that potash, melted with the cerite and dissolved in water, did not give any colour.

Component parts of the ore, 50 per cent. of oxide of cerium.

Not having the practice which complete proportionate analyses require, we offer these results with diffidence, and in the hope that scientific men of more experience will employ themselves on this subject.

§ III.

Examination of the Metallic Oxide found in the Cerite.

A PASTE was made of 37 grains of this oxide and linseed oil, which was reduced in charcoal in a covered crucible. Although it retained some carbon, it lost half a grain of its weight. This mass was inclosed in a lined crucible with-

Examination of the oxide.

out

out any flux, and M. Hjelm expofed it for half an hour to the degree of fire neceffary for the reduction of manganefe. The oxide was not melted, but reduced into a very fine powder: it exhibited brilliant particles in the day-light, and ftained white paper black. It diffolved in muriatic acid, difengaging, at the firft, fulphurated hidrogen gas, and afterwards pure hidrogen gas. This colourlefs folution had a faccharine tafte. Thus it appeared to us that the metal was reduced in part. The origin of the fulphur may be traced to the fulphuric acid, from which the matter had been feparated by the cauftic ammonia. The influence which this acid exercifes in thefe affays, will be feen by the fubfequent enquiries.

M. Gahn, at Fahlun, having more convenient furnaces, has promifed to undertake the reduction of this fubftance with more power: if this operation fucceed, we fhall give an account of it hereafter.

It is the oxide of a metal not yet known.

Thefe appearances, and thofe which follow, determined us to confider the fubftance found in the cerite, as the oxide of a metal hitherto unknown, to which we have given the name of Cerium, from the planet Ceres, difcovered by Piazzi.

Manner of obtaining the pure Oxide of Cerium.

Proceffes for obtaining the pure oxide.

(A.) Pure uncalcined cerite was diffolved in nitro-muriatic, and, after faturating the clear folution with the alkali, was precipitated by tartrite of potafh. The precipitate well wafhed, calcined, and digefted in vinegar, contains the pure oxide of cerium.

Or otherwife decompofe a folution of cerium in nitro-muriatic acid, ftill warm, but not faturated, by fuccinate of ammonia: a fuccinate of iron is gradually depofited. The precipitation is to be continued by means of fuccinate of ammonia, as long as a red precipitate is formed: the folution is then nearly deprived of iron. After having feparated the fuccinate of iron, more fuccinate of ammonia is poured into it, until a white precipitate appears. The folution is then left at reft, in order that the fmall portion of fuccinate of cerium may be depofited. The iron diffolved by the free muriatic acid, is depofited at the fame time, and the folution is freed from this metal. The cerium may afterwards be precipitated by ammonia, and then wafhed and calcined.

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Of the Properties of Oxide of Cerium.

(B.) This oxide may appear in different degrees of oxida- The oxide is tion. The alkalies precipitate a white oxide from its folutions, capable of different degrees of which fhows of a yellowifh colour in the air, but, when per- oxidation. fectly dried, becomes dark. Expofed to a brifk and long-continued fire, it takes a deep brick-colour. The oxalate and acetate of cerium, calcined in veffels not completely clofed, yield a white oxide, which, in an open fire, becomes of the colour of bricks. It does not melt by itfelf.

Treated with borax by the blow-pipe, it melts readily and Fufion with fwells. The globule heated by the exterior flame, affumes fluxes. the colour of blood; which, by cooling, paffes to a yellowifh green, and at length becomes colourlefs, and perfectly tranf-parent. Melted by the interior flame, thefe changes do not take place; it is then reduced into a colourlefs glafs; but, ex-pofed for a fhort time in the exterior flame, the fame pheno-mena are produced. If too much oxide of cerium is made ufe of, the glafs refembles an opake yellowifh enamel. Thefe changes are more eafily manifefted with the phofphate of foda and ammonia. If two clear and colourlefs globules are melted together, one of which was prepared with borax and the other with the phofphate, they form a tranfparent glafs, which, on cooling, becomes opake and pearl coloured.

Thefe characters, taken together, fufficiently diftinguifh the oxide of cerium from the oxide of iron. The latter alfo offers the fame changes of colour, but its glafs, on cooling, has a deep green colour, *which fades*. The globules made with borax and the phofphate melted together, yield an opake glafs, the colour of which is a little deeper.

Oxide of Cerium treated with Sulphuric Acid.

(C.) When oxide of cerium is digefted with fulphuric acid, Sulphate of ce-thefe two fubftances unite, and the refult is a red infoluble rium at a maxi-falt, which is fulphate of cerium at a *maximum of oxidation*. tion. If the acid is concentrated, it fcarcely diffolves any of it. If it is diluted with half its quantity of water, or a little more, the refult is a yellowifh oily liquor, which does not adhere to the glafs, nor does it wet it. If the acid is mixed with fix or feven times its quantity, or even more, of water, and em-ployed in a fufficient quantity, the oxide is diffolved of an orange

orange colour., By a flight evaporation of this folution, It yields fmall prifmatic, coherent cryftals, of the colour of gold. This falt is an acidulous fulphate of cerium at a *maximum*. Thefe cryftals, thoroughly dried between blotting-paper, and expofed to the air, are gradually reduced to a yellow, almoft cryftalline powder. Rediffolved in water, they experienced a decompofition; a white powder is depofited, and the folution becomes colourlefs. This white powder is a fulphate of cerium, but little oxigenated. If the folution is evaporated to drynefs, it gives an acidulous fulphate of cerium ftill lefs oxigenated. Thefe cryftals are feldom cubical, but almoft always prifmatic, ftriated and collected in bundles. Their tafte is four, but they afterwards become faccharine and aftringent.

Treated with muriatic acid, the yellow acidulous fulphate of cerium yields part of its oxigen to the acid, which is volatilized in oxigenated muriatic acid gas. The falt remains colourlefs. An augmentation of temperature alone is fufficient for the yellow acidulous fulphate of cerium to lofe its colour by lofing the excefs of its oxigen. If the heat is increafed ftill more, the furplus of the acid is carried off, and a faturated fulphate of cerium remains. By a continued calcination, it regains oxigen, becomes red, and yields a fulphate of cerium at a *maximum*. The fulphate of cerium, difoxigenated by the muriatic acid, is more difficult to re-oxidate by calcination.

In the humid way, the alkalies only decompofe the fulphate of cerium incompletely. Ammonia precipitates an oxide from the acidulous fulphate of cerium, which is only in a fmall quantity, but neverthelefs contains part of the fulphuric acid. The fulphate of cerium is not perfectly decompofed, except by calcination with three times its weight of carbonate of foda or potafh. The calcined oxide is of a brown colour. By digeftion, ammonia can deprive it of part of its acid: the oxide takes a diftinct flefh colour, which becomes brighter by drying. Digefted with concentrated muriatic acid, or with nitric acid, a fmall quantity diffolves, having its yellow colour.

If a folution of acidulous fulphate of cerium is precipitated by potafh, a triple combination of cerium, fulphuric acid, and potafh, is feparated, before the acid is faturated. If too much potafh is added, the combination is partly deftroyed. The fulphate of cerium and potafh, at a *maximum*, is of an orange colour;

Acidulous fulphate of cerium.

Neutral acid.

Acidulous fulphate of cerium but flightly oxigenated.

**Muriatic acid deprives the acidulous fulphate of part of its oxigen:
As does an increafe of temperature, which afterwards drives off the acid, and forms a neutral fulphate.
Calcination reftores the oxigen.**

Action of the alkalies on the fulphates of cerium.

Sulphate of cerium and potafh.

colour ; that which is at a *minimum*, is white. A fimilar com-
bination is alfo obtained by pouring muriate of cerium into a
folution of fulphate of potafh. Sulphate of ammonia does not
form any precipitate in it ; but, on adding a calcareous falt
to it, fulphate of cerium combined with potafh is rapidly
depofited.

Thefe characters offer a ready method of feparating the ful-
phate of cerium from iron. It muft however be obferved,
that when the folutions are faturated, a little iron is alfo de-
pofited, which gives a yellow colour to the precipitate; but
by adding a little fulphuric acid in excefs, the iron is redif-
folved, and leaves the precipitate entirely white. This com-
bination is only diffolved in part by dilute fulphuric acid, and
the greateft quantity of that feparates afterwards. *{Sulphuric acid in excefs frees fulphate of cerium from iron.}*

The fulphate of cerium and potafh melts by a ftrong heat.
Feated with charcoal, it gives fulphuret of potafh and ful-
phuret of cerium. Melted with carbonate of potafh, in clofed
veffels, it yields carbonate of cerium and fulphate of potafh.
This falt contains only one-third of oxide of cerium. *{Sulphate, and carbonate of cerium.}*

It is diffolved in concentrated nitric acid, and, during the
cooling, an acid falt, formed of acidulous fulphate of potafh
and a little fulphate of cerium, cryftallizes. Thus the fulphate
of cerium combined with potafh, is decompofed by concen-
trated nitric acid. This acid carries off the metal, and the
fulphuric acid is directed wholly upon the potafh, with which
it forms a falt with excefs of acid. *{Concentrated nitric acid de-compofes ful-phate of cerium and potafh.}*

Oxide of Cerium with Nitric Acid.

(D.) Nitric acid diffolves the calcined oxide with difficulty,
but that which is precipitated by pure or carbonated alkalies,
with eafe. When the folution is faturated with oxigen, it is
of a greenifh yellow colour ; but colourlefs, when lefs oxided.
Evaporated to the confiftence of honey, it depofits lamellated
cryftals, which attract the humidity of the air. The folution
has a faccharine tafte : like all the other faturated folutions of
cerium, it lets fall an oxide of cerium, at a *maximum* of oxi-
dation, in the open air. This precipitate is frequently formed
of oxide of iron. When dry, this falt is of a yellowifh white
colour ; but becomes colourlefs on being diffolved in a fuffi-
cient quantity of water. It diffolves readily in alcohol. *{Nitrate of ceri-um.}*

4   A con-

A concentrated folution of this falt takes a blood colour, on account of a fmall quantity of iron, which, by drying, paffes to yellowifh white, but is reftored by a new folution.

A colourlefs and lefs oxided nitrate of cerium, is obtained by diffolving the yellowifh falt in alcohol: the folution inflames and yields a white falt.

It is deftroyed by fire, which drives off its acid.

With Muriatic Acid.

Muriate of cerium.

(E.) The calcined oxide of cerium is flowly diffolved in muriatic acid in the cold, and more readily by heat ; an effervefcence is produced, owing to a difengagement of oxigenated muriatic acid gas. The tafte of the folution is faocharine and aftringent; the colour is a very faint greenifh yellow : the dried faline mafs is yellowifh white, and attracts humidity. We only fucceeded once in obtaining it cryftallized. The cryftals were white, brilliant, in four fidéd prifms, with the points cut off. The falt diffolves readily in alcohol, and its concentrated folution burns with a yellow and fparkling flame. The refidue of the falt is white and gives a colourlefs folution. It is muriate of cerium at a *minimum* of oxidation.

Heated in clofed veffels, the water of cryftallization is firft diffipated, afterwards the acid paffes in the form of oxigenated muriatic acid gas. If the operation is ftopped before the acid is entirely volatilized, an undecompofed muriate of cerium, at a *minimum* of oxidation remains.

If the muriate of cerium contains iron, it all fublimes in a brown deliquefcent mafs. Nothing remains in the matrafs but a white oxide of cerium, which attracts the humidity of the air, and becomes yellow. Thus, fublimation with muriate of ammonia may be employed to purify a muriate of cerium which contains a little iron.

With Phofphoric Acid.

Phofphate of cerium.

(F.) Free phofphoric acid, faturated with an alkali, precipitates muriate of cerium. The precipitate is white, and foluble in muriatic acid and in nitric acid employed in fufficient quantity.

This falt is alfo obtained by digefting pure oxide of cerium, moiftened with phofphoric acid. It is not foluble in an excefs of this acid.

With

With Carbonic Acid.

(G.) The carbonate of ammonia precipitates muriate of cerium without effervefcence. After the precipitation, carbonic acid is flowly difengaged in the form of bubbles. The refidue retains its acid, even after deficcation.

Carbonate of cerium.

Dry carbonate of cerium has a white colour tending a little to bluifh or greenifh. It diffolves in the acids with effervefcence. It does not lofe its acid in an open fire. In clofed veffels, without the contaƐt of oxigen, it fupports a gentle calcination, without being decompofed.

With Arfenic Acid.

(H.) Free arfenic acid does not produce any change on muriate of cerium. The oxide digefted with this acid, forms an infoluble falt. An excefs of this acid diffolves it, and gives an acidulous arfeniate of cerium. The faturated arfeniate of cerium is depofited in the form of a powder during the evaporation. The refidue does not cryftallize, but by deficcation, becomes a gelatinous, clear, and colourlefs mafs.

Arfeniate and acidulous arfeniate of cerium.

With Molibdic Acid.

(I.) The acidulous falts of cerium are not decompofed by molybdate of ammoniac. The molybdate of cerium is precipitated from its faturated folutions, in the form of a white falt, which is not foluble in the acids.

Molybdate of cerium.

With Oxalic Acid.

(K.) Either the acidulous or faturated folutions of cerium are precipitated by oxalic acid. According to the degree of oxidation of the metal, the precipitate becomes red or white. This combination is alfo obtained by digefting the oxide with free oxalic acid. An excefs of acid does not diffolve it, but ammonia readily effeƐts its folution, giving it a yellow colour.

Oxalate of cerium.

A fmall quantity of oxide is depofited by evaporation. The folution afterwards yields regular cryftals in the form of needles. Pure alkalies do not occafion any precipitate.

With Tartareous Acid.

Tartrite of cerium.

(*L.*) Free tartareous acid has no action on muriate of cerium. The recently precipitated oxide unites with this acid by digestion, and yields a tartrite of cerium, which diffolves readily in water. This falt is alfo precipitated from faturated folutions by tartrite of potafh. Like the oxilate of cerium, it diffolves in pure ammonia, but does not cryftallize.

Tartrite of cerium is not entirely foluble in water; the folution is precipitated by carbonate of foda.

With Benzoic Acid.

Benzoate of cerium.

(*M.*) This acid does not act on the muriate of cerium; but, by digestion, well concentrated benzoic acid diffolves the oxide of cerium recently precipitated. On cooling, the folution firft depofits cryftals of the acid in excefs, and afterwards benzoate of cerium in the form of a white powder, which adheres to the cryftals of benzoic acid.

The refinous matter with which this acid is frequently united, combines with the benzoate of cerium, and forms an infoluble brown powder.

With Citric Acid.

Citrate and acidulous citrate of cerium.

(*N.*) Muriate of cerium is not precipitated by citric acid. But the oxide digefted with citric acid forms an infoluble faturated combination, which an excefs of acid diffolves. The acidulous citrate of cerium is of a yellow colour and does not cryftallize. Alcohol deprives it of its water, and of part of its acid, but does not diffolve it.

With Acetic Acid.

Acetate of cerium.

(*O.*) The calcined oxide of cerium is only very imperfectly diffolved in acetic acid, even with the affiftance of heat; but that which is recently precipitated by the alkalies, is diffolved with facility. The faturated acetate of cerium is foluble in water; it has a fweet tafte, and gives granulated cryftals, by evaporation, which do not change in the air, and are but flightly foluble in alcohol.

This falt fwells in the fire, and is deftroyed.

(*To be continued.*)

Dutch-proof, from five pounds of the dried fruit. This brandy, although it retains a flight odour of the fruit, is by no means difagreeable to the tafte, and I have made liqueurs with it in no refpect inferior to thofe of commerce. I fhall not delay the publication of the details of this difcovery, from which I claim no credit except on account of the influence which it may have on a part of the commerce of the eaftern provinces of Spain.

Method of obtaining pure Cobalt. By TROMSDORFF [*].

LET four parts of finely pulverized zaffre be intimately blended with one of very dry nitrate of potafh and half a part of charcoal powder; introduce this mixture by fmall quantities at a time into an ignited crucible, and repeat this procefs for three fucceffive times, by again adding to the detonated refiduary mafs new quantities of nitrate of potafh and charcoal. This being done, mix the mafs with one part of black flux, and expofe the mixture to a red heat for one hour. When cooled, feparate the metallic cobalt, pulverife it, blend it together with three times its own weight of nitrate of potafh, and detonate this mixture as directed before. The iron which was prefent will thus became highly oxidized, and the arfenic acidified, and combined with the potafh. To feparate the latter, pulverife the mafs, treat it repeatedly in water, and feparate the infoluble part by the filtre. The arfeniate of potafh being thus got rid of, digeft the refidue in nitric acid; the cobalt will now be diffolved, and the highly oxidized iron remain untouched. Evaporate the folution to drynefs, rediffolve it in nitric acid, re-filtre the folution, in cafe fome oxide of iron fhould have efcaped unfeparated, decompofe the folution of nitrate of cobalt by potafh, wafh the precipitate, and reduce it by heat.

Puie cobalt.

Method of coating Copper with Platina. By STRAUSS.

Mr. Straufs, who has made a number of good experiments on platina, has fucceeded in applying this metal to defend the face of copper. The folution of platina was precipitated by

Coating copper with platina.

muriate

muriate of ammonia, then wafhed and dried, and expofed to a graduated red heat for half an hour in a covered crucible. The produ&t was a grey coagulated powder; confifting of metallic platina in a ftate of extreme divifion. One part of this powder with three parts of mercury did not combine by half an hour's triniration; but upon adding two parts more of mercury and flightly heating the mortar, he foon obtained a tenacious amalgam, which was rendered very foft by the addition of two other parts of the laft mentioned metal.

A fmall quantity of this amalgam was rubbed upon a plate of copper, which became perfe&tly covered. The plate was then ignited, and was found to have retained a coating of platina. In the next place he mixed a little of the amalgam with chalk, fprinkled the mixture with water, coated the plate of copper a fecond time, and again ignited it. The coating was now found to be very perfe&t, and affumed a fhining filver colour under the burnifher.

This chemift remarks that his application of platina to copper veffels muft be fuperior to that of tin; not only in its refiftance to acids and faline matters, but in its durability from the greater hardnefs of platina; and he adds that the procefs here defcribed, is not more difficult to be effe&ted than the common operation of tinning.

Tubular Pendulum by Mr Edward Troughton.

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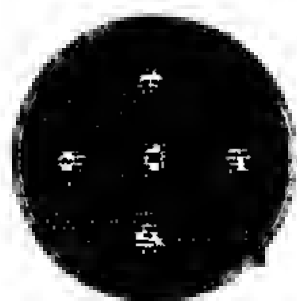
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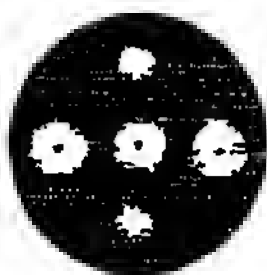
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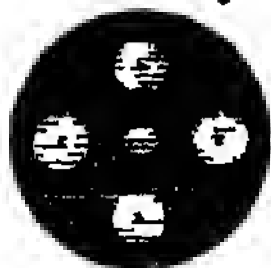
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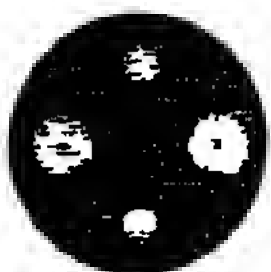
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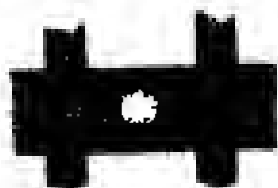
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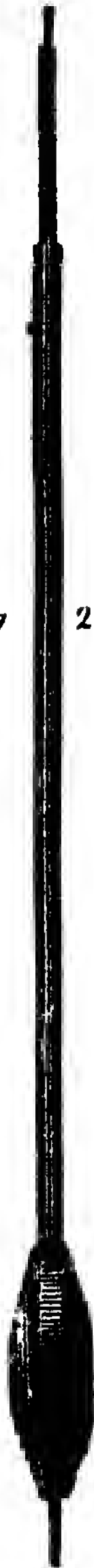
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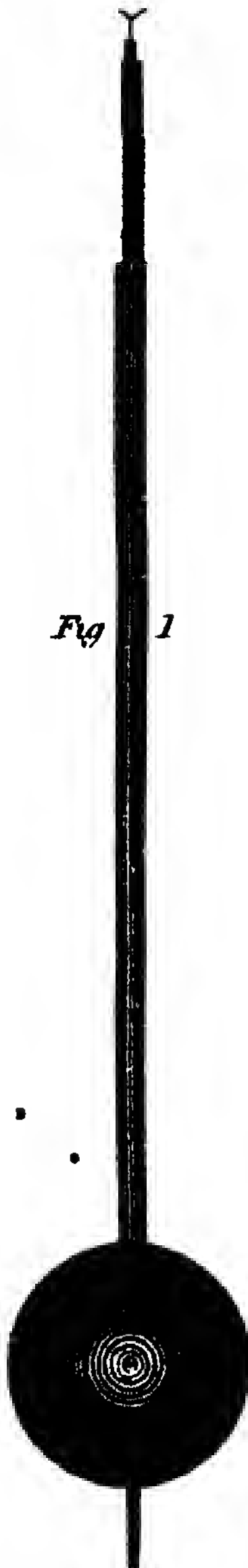
Instrument for measuring the Absorption of the Gases, by Charcoal by C.L. Morozzo.

Fig 1
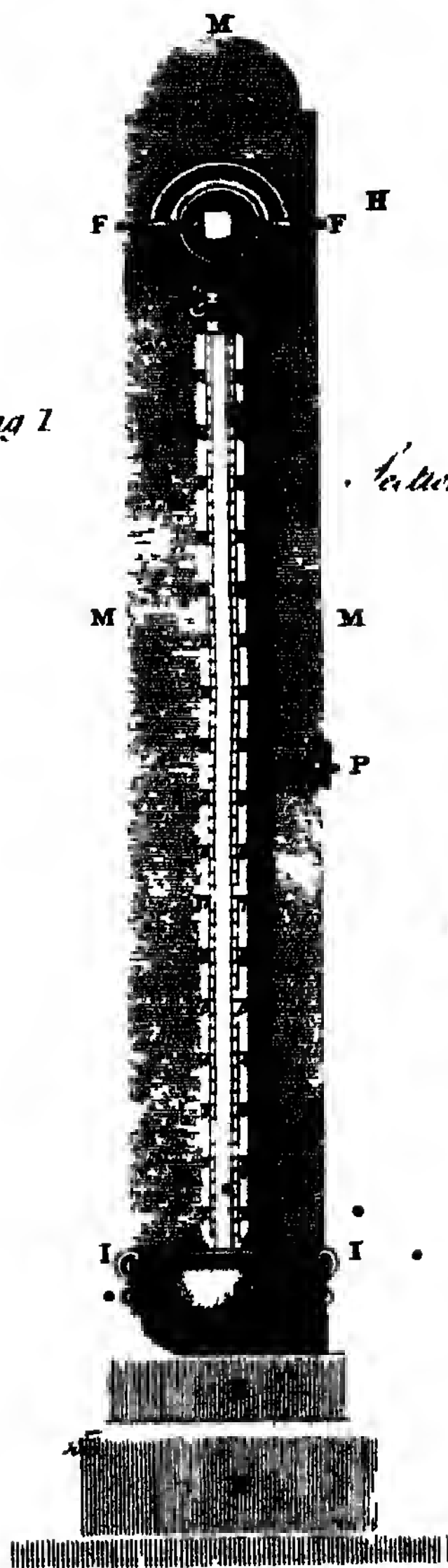
Section in the Line AB

Fig 3

Fig 2

(ELEVATION)

Combiner of Forces by H. Frang.

(PLAN)

Quadrantum

Fig. 2

INDEX.

INDEX.

INDEX.

INDEX

END OF THE NINTH VOLUME.

ERRATUM.

In *Plate* XII. *Fig.* 3, by an omiffion in the defign, the lower ruler has not the fame connection with the frame as the upper. It muft be joined by prolonging the rod from the left hand angle, and adding a rod from thence to the bar upon the ruler; as is fhewn above.